THE PHILOSOPHICAL MAGAZINE,

OR

ANNALS

OF

CHEMISTRY, MATHEMATICS, ASTRONOMY, NATURAL HISTORY, AND GENERAL SCIENCE.

BY

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AND

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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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ALERE FLAMMAM

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PLATES.

ERRATA.

Page 218, line 24, *for* doubt *read* dwell.
Page 245, line 1, *for* plane *read* plain.
Page 285. The first paragraph in this page should read as follows:—
"both of reflections and shadows, and some problems in *inverse* perspective (i.e. the making out the geometric form from the perspective one, as well as the perspective from the geometric form), would be desirable additions. A table of contents, or rather an index, would also improve this Supplement, and will we hope be added to a future edition; for although the work is divided into chapters," &c.

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

JULY 1830.

I. *On the Measurement (by Trigonometry) of the Heights of the principal Hills of Swaledale, Yorkshire.* By JOHN NIXON, Esq.*

[With a Plate.]

AVAILING myself last year of the favourable state of the weather about midsummer, I undertook, and within three weeks finished, the Trigonometrical Survey of the Hills of Swaledale, a valley north of, and adjoining that of Wensleydale.

It was originally intended to have placed signals on the principal hills only (the peculiar features of the dale rendering impracticable the plan adopted in the preceding survey); yet the temptation in the course of the operations, of marking for measurement such others of minor note as were situated on our route, could seldom be resisted. The exceptions had at length become so reduced in number, that the deficient signals would have been supplied, and the survey made complete for hills of both descriptions, had not numerous indications of approaching tempestuous weather urged the propriety of commencing the observations without further delay. As an additional motive to dispatch, it was noticed during the progress of the measurements that two of the more distant signals had successively disappeared; one of which, on West Stonesdale Moor, having been observed from one station only, was totally lost to the survey. The other, on the Nine Standards Hill, had most probably been demolished by a jealous shepherd or keeper, acting under the impression that it had been set up in opposition to the adjacent nine *standards* or boundary marks between Westmoreland and Yorkshire, whence the fell derives its name. As the line of demarcation passes in this vicinity through every point on the ridge common to the geological

* Communicated by the Author.

troughs of the Eden and the Swale, it would at once be admitted that our signal, raised on the extreme summit of the hill, stood on the precise line of division between the two counties, and that the standards, placed a quarter of a mile to the north-west on ground considerably lower, were unequivocally within Edendale; but on descending the east side of the hill, we should discover that a stream, deriving its supplies principally from ground in the direction of the standards, divides and pours its waters into *both* valleys. To fix permanently a boundary evidently debateable, has no doubt led to the building of these numerous and enormous pikes. To prevent the destruction of the second signal, recesses were made within two of its opposite sides, each furnished with a stiff card, on which was explained (in pencil, to avoid obliteration) the occasion of its erection. From the effects of a similar jealousy the measurement of the heights of the Dent Hills, commenced in 1824, at which period the boundaries between Dent and Newbyhead were in dispute, was obliged to be abandoned for two years; the signals within the contested ground being regularly destroyed, though repeatedly renewed, and every particle of the materials removed from the place.

The inferior hills of Swaledale not included in the survey, are the following. 1. West Stonesdale Moor; the loftiest eminence on the boundary ridge between the Nine Standards and the pass at Tanhill. 2. Angrain Moor; one of the many ridges of which Shunnor Fell is the common crest; eligible for measurement or otherwise, according as the ridge previous to its termination in a knab is interrupted by a depression, or more probably forms one continuous declivity. 3. Windyates; a transverse bank of the boundary ridge (crossed by the road from Askrigg to Reeth), inferior in altitude to either High Fleak or Pickington Ridge, between which it is situated without the intervention of a regular pass, but well known from the bold cliff of the top or twelve-fathom lime at its southern extremity. 4. Mirk Fell; partly in Teesdale, a dependent of and considerably lower than Water Crag, from which it is scarcely sufficiently distinct to merit a separate appellation. 5. Healaugh Crag; a mass of grit rock breaking the uniformity of a broad ridge falling from Great Pinch Yate, in the direction of the village of Feetham. 6°. Windegg; a slight cliff of the twelve-fathom lime at its termination on the boundary ridge under the grit of the Hoove. 6. Hurst Moor: and 7. Fremington Edge; together forming a lofty escarpment of the same and other limes on the left bank of the Arkle. From the Hoove to Fremington the eastern ridge of Arkendale is one uninterrupted and nearly uniform descent, possessing no other claim

claim on the survey than its celebrity as an extensive and most productive mining field. (Having finished the signal on the Hoove in the early part of the day, it was intended to have descended the ridge and marked two or more conspicuous points for measurement; a design unexpectedly frustrated by the occurrence of a local storm of wind and dense mist.) Lastly, there appears, according to the shading of the maps, a round eminence, marked as Byer's Hill, situated considerably to the east of the Hurst Moor. From the station on Pinch Yate, which affords a commanding view of that part of the dale, the hill, probably from its insignificancy, could not be distinguished.

Conical piles of large sods, constructed of the usual dimensions, served for signals on Hugh Seat, Nine Standards, Keasdon, East Stonesdale Moor, Rogan's Seat, Blake Hill, Brownsey, Great Pinch Yate, the Hoove, and Gibbon Hill; smaller piles of stone marking the summits of the Tail Brigg, Calvey, Harker, and Grinton Grits. Of these hills the highest point was most carefully determined by the (repaired) twelve-inch telescopic-level. About half a mile S.S.W. of the signal on the Nine Standards is an eminence so nearly of equal altitude, that it was deemed necessary to repair there in order to confirm by reciprocal observation the correctness of the adjustments of the level. With a view to expedite the completion of the signals required, as well as to guard against their future demolition, parties of miners were hired as frequently as possible to procure and pile the materials on the exact spot marked out. At Dod End and Satron Hangers it was unavoidably necessary to entrust to them, not only the erection of the signals, but also the fixing of their sites; a task which the extensive practice of the miners in forming watercourses would render them competent to execute with the requisite accuracy. That these signals were properly placed was verified by applying to them the test of the horizontal wire of the horizon-sector from several stations of about the same altitude, in which case the true and apparent (or perspective) summits would sensibly coincide in level. The highest point of Robincross Hill, at a right-angled turn of the wall crossing its ridge, was marked by placing on the wall an adequate number of loose stones. Holgate Pasture was not visited; but from its round figure, inconsiderable size, and remote distance, it scarcely required a signal.

The difficulty of correctly bisecting a signal by the vertical wire of the telescope of the theodolite, hitherto effected by the tedious perfecting of an approximate bisection, was finally surmounted by the following mode of operation. Commencing at once with the wire placed at an angle of some minutes to the

left of the object, the tangent screw was turned in the proper direction at one uniform, somewhat rapid rate, until the wire appeared to have reached the middle of the signal, when the screw was abandoned as instantaneously and abruptly as possible. Bisections of this description are not only permanently exact, but have the advantage of being completed with great œconomy of time; a consideration of great importance in variable weather. The method, though difficult at first, requiring a quick eye and considerable address in the manipulation of the tangent screw, becomes with practice mechanically easy and certain. If we estimate the probable angular error of bisection inversely as the distance of the objects, it must be allowed, on a comparison of the subjoined list with the corresponding one for Wensleydale, (regard being had to the greater magnitude of the triangles of the latter,) that the observations of 1829 are decidedly superior to those of the year preceding it.

List of the difference of the sum of the angles of every triangle, of which all the angles have been observed, and 180°.

−0′ 2″	−0′ 53″	+0′ 5″	+0′ 46″
0 3	1 2	0 9	0 47
0 4	1 2	0 10	0 58
0 19	1 7	0 12	1 5
0 21	1 10	0 19	2 3
0 24	1 14	0 30	
0 43	1 26	0 39	

Sum of errors (disregarding signs) 17′ 33″, or 13″ per angle.

The loftiest of the Nine Standards and the boundary pikes on Shunnor Fell and Water Crag are signals of the Ordnance Survey, and form the extremities of three base-lines, sufficient in number and advantageously situated for a basis to the survey; but on calculating the approximate distances required to obtain the corrections for the excentricity of the theodolite, it was discovered that the given distance of the highest standard to Water Crag must be several hundred feet in excess. On this account it was considered proper to connect the triangulation with other stations of the Ordnance Survey, which was effected in the autumn following, on the occasion of the measurement at Ingleborough of some angles relating to operations in another quarter. As the high opinion entertained of the accuracy of the distances of the Trigonometrical Survey has justly rendered them the foundation of county and other extensive surveys, it becomes a duty to point out such as have been discovered to be indisputably incorrect.

At Water Crag the horizontal angle between Shunnor Fell and the highest standard measured 52° 47′ 56″, or 1′ 36″ less than

than as observed by Colonel Mudge*; the trifling difference arising from the imperfection of the small theodolite added to some uncertainty as to the precise site of the original signals, rather than from the want of identity in the object bisected by the two observers. This is sufficiently obvious from the circumstance of the Nine Standards being placed in a line of at least fifty yards in extent, nearly fronting Water Crag, and on the very extremity of its horizon in that direction. To remove any unreasonable doubt of identity founded on the idea that the ambiguous term "*the highest pile on the Nine Standards*" does not absolutely or exclusively apply to the loftiest of the standards, it may suffice to observe that the other erections in their vicinity are all situated to the west of, and considerably below the level of a long transverse bank (terminating to the north at the Standards), which completely interrupts the view of them from Water Crag. The loss of the original signal on the Nine Standards having been first noticed from Water Crag, I have a perfect recollection of the anxiety with which the telescope was repeatedly directed to every part of the edge of this bank, in the vain hope of distinguishing some vestige of the signals: nothing whatever could be seen rising above the level of the heath. Having clearly proved that the loftiest of the Nine Standards was the object alluded to by Colonel Mudge, it is equally demonstrable, on the authority of measurements derived from two independent bases (of which the results are given in the calculation of the distances), that his statement of its distance from Water Crag exceeds the truth by about 697 feet. The correctness of the observation from Water Crag being established, the error in *excess* in the distance must have been committed in the corresponding observation from Cross Fell, at which station it would appear that some objects to the *west* of the Nine Standards had been mistaken for them. This is the more probable, as the boundary ridge in that quarter of the fell and the adjoining coal-field are literally studded with piles of stone of every description. It would, in fact, require

* The following are the only particulars relating to the measurement of the disputed distance contained in the Account of the Survey.

Cross Fell to Water Crag 125742 feet.

Water Crag	93° 12′ 26″	35705 feet.
Cross Fell	15 35 36	132621
Shunnor Fell	(71 11 58)	
Cross Fell	13 7 52	101367
Water Crag	40 26 6	35506
Highest Pile on the Nine Standards	(126 26 2)	

a most

a most favourable distribution of light and shade to render the dull masses of the standards distinguishable, at a distance of twenty miles, from the dark sides of the ridge immediately behind them.

The horizontal angles were all measured on the six-inch theodolite already described, and generally in opposite directions of the telescope within its Ys, of which the mean readings, corrected for the distance of the instrument from the signal, are given in the Registers.

In the event of the measurement of a distance from more than one base, the mean of the results, when not sufficiently accordant with each other, has been determined by the formulæ given in the Phil. Mag. and Annals, N.S. vol. v. page 354.

Register of the Measurement of the Horizontal Angles.

At Shunnor Fell.

	Readings.		
Water Crag	0°	0′	0″
Rogan's Seat	3	8	42
Keasdon	15	23	38
Great Pinch Yate	18	20	1
Blake Hill	23	5	34
Dod End	23	27	23
Brownsey	23	55	6
Harker	42	22	45
Satron Hangers	48	12	6
Pickington Ridge	52	30	17
Bakestone Edge	61	5	32
Great Whernside	99	4	20
* Pen-y-gent	134	45	16
Dod Fell	135	43	47
Knoutberry Hill	162	58	15
Ingleborough	157	39	25
Pillar Hill	247	6	5
Hugh Seat	262	53	31
The Tail Brigg	282	58	17
Nine Standards Hill	297	37	47
East Stonesdale Moor	350	22	51

At Water Crag.

Rogan's Seat	10°	12′	52″
† Ingleborough	10	20	54
Shunnor Fell	25	58	58
Hugh Seat	53	33	20

	Readings.		
* Nine Standards Hill	76°	29′	32″
Highest Standard	78	46	54
The Hoove	230	48	47
Great Pinch Yate	273	25	43
‡ Whitfield Hill	280	56	15
Robincross Hill	282	10	5
Gibbon Hill	293	57	16
Pickington Ridge	304	55	12
Brownsey	307	4	19
Satron Hangers	321	9	31
† Great Whernside	324	57	31
Settronside	331	16	43
Blake Hill	338	9	12
Bakestone Edge	340	54	17

At Ingleborough.

Shunnor Fell	0°	0′	0″
† Water Crag	6	42	28
Bakestone Edge	17	0	45
Great Whernside	66	10	27
Hugh Seat	347	31	52

At Bakestone Edge.

Keasdon	0°	35′	46″
East Stonesdale Moor	12	3	56
Rogan's Seat	23	19	40
Water Crag	29	31	18

Blake

	Readings.		
Blake Hill	31°	49′	15″
Brownsey	51	46	35
Great Pinch Yate	56	36	26
**Holgate Pasture	81	53	36
Calvey	84	43	29
Satron Hangers	90	57	23
Harker	101	41	32
Pickington Ridge	117	15	41
Great Whernside	186	31	33
Ingleborough	249	17	38
Shunnor Fell	315	42	41
Hugh Seat	325	6	41
The Tail Brigg	338	14	48
Nine Standards Hill	345	55	49

At Pickington Ridge.

Gibbon Hill	27°	45′	57″
Grinton Grits	48	35	15
Robincross Hill	59	6	19
‡ Whitfield Hill	66	1	23
Settronside	167	23	38
High Fleak	240	31	25
Bakestone Edge	245	41	54
Lovely Seat	247	12	54
Shunnor Fell	255	33	22
Hugh Seat	264	18	0
Satron Hangers	266	28	10
Dod End	277	12	2
Nine Standards Hill	279	26	29
Highest Standard	280	20	7
Blake Hill	285	4	32
Rogan's Seat	293	27	7
Brownsey	300	40	16
Water Crag	301	59	24
Great Pinch Yate	318	22	3
The Hoove	338	45	41
Calvey	353	45	17

At Great Pinch Yate.

Settronside	7°	0′	21″
High Fleak	14	18	28
Satron Hangers	17	48	5
Bakestone Edge	34	28	43

	Readings		
Brownsey	46°	48′	57″
Lovely Seat	54	46	40
Blake Hill	62	25	26
Shunnor Fell	70	48	55
Rogan's Seat	98	50	44
Water Crag	119	55	4
The Hoove	221	48	31
* * Holgate Pasture	262	59	12
Calvey	303	44	20
‡ Whitfield Hill	309	49	47
Robincross Hill	311	56	49
* Harker	322	51	42
Grinton Grits	325	6	54
Gibbon Hill	332	3	45
Pickington Ridge	347	47	2
Great Whernside	358	29	35

At Harker.

* Bakestone Edge	109°	3′	5″
Satron Hangers	114	37	55
Great Pinch Yate	172	19	56
Calvey	204	11	44
The Hoove	204	20	20
* * Holgate Pasture	247	27	2
Robincross Hill	325	28	19

At Keasdon.

East Stonesdale Moor	75°	12′	10″
Blake Hill	158	57	37
Dod End	173	12	47
Bakestone Edge	227	17	41
Lovely Seat	276	21	41
Shunnor Fell	316	41	28
Hugh Seat	351	55	0

At Nine Standards Hill.

The Tail Brigg	40°	1′	45″
Highest Standard	139	13	30
Water Crag	245	59	15
East Stonesdale Moor	254	22	30

	Readings.		
* Pickington Ridge	271°	52′	32″
Keasdon	278	8	7
Bakestone Edge	286	48	30
Shunnor Fell	313	6	30
Hugh Seat	353	35	5

At Blake Hill.

Water Crag	52°	47′	52″
Great Pinch Yate	110	34	55
Pickington Ridge	182	39	46
Satron Hangers	199	26	16
Bakestone Edge	237	50	40
Dod End	301	59	31
Shunnor Fell	303	44	5
Keasdon	318	17	26
Hugh Seat	328	13	16

	Readings.		
Highest Standard	354°	37′	47″

At Calvey.

Satron Hangers	4°	35′	54″
Bakestone Edge	7	33	31
Lovely Seat	18	22	58
Brownsey	43	56	9
Great Pinch Yate	68	41	3
The Hoove	119	53	40
‡ Whitfield Hill	258	1	51
Robincross Hill	262	33	54
Grinton Grits	291	50	57
Harker	299	40	13
Gibbon Hill	310	35	46
Pickington Ridge	328	7	3
High Fleak	353	35	44

Explanation of the Signs.

* denotes that the reading was not that of an actual observation, but obtained by the method explained at page 353, vol. v.

** implies that there was no signal erected; and ‡ that the one set up had been destroyed.

Readings procured from data of the Ordnance Survey are marked †.

Calculation of the Mean Distances.

Bases from the Ordnance Survey.

		Feet.
I.	Shunnor Fell to Ingleborough	82397
II.	——— Water Crag	35705
III.	——— Great Whernside	91758
IV.	Water Crag to Ingleborough	116216
V.	——— Great Whernside	103573
VI.	Great Whernside to Ingleborough	85598

Bakestone Edge to Ingleborough.

	Feet.
By Inglebro' and G^t Whernside	89303
——— Shunnor Fell	14
——— Water Crag	37
Mean	89316

Bakestone Edge to Shunnor Fell.

By Shunnor Fell & G^t Whernside	26297
——— Ingleborough	307
——— Water Crag	328
Mean	26319

Bakestone Edge to Water Crag.

	Feet.
By Water Crag & Shunnor Fell	32548
——— Ingleborough	26
Mean	32545

Bakestone Edge to Great Whernside.

By G^t Whernside & Inglebro'	72833
———Shunnor Fell	51
Mean	72842

Great

Great Pinch Yate to Shunnor Fell.

	Feet.
By Shun. Fell & Bakestone Edge	43613
——— Water Crag	21
——— Great Whernside	35
Mean	43621

Great Pinch Yate to Water Crag.

By WaterCrag and Shunnor Fell	14862
——— BakestoneEdge	65
——— Gt Whernside	87
Mean	14866

Great Pinch Yate to Bakestone Edge.

By Bakest. Edge & Shunnor Fell	30154
——— Water Crag	58
Mean	30156

Pickington Ridge to Shunnor Fell.

By Shunn. Fell and Water Crag	48679
——— Pinch Yate	48679
Mean	48679

Pickington Ridge to Water Crag.

By Water C. & Bakestone Edge	39094
——— Pinch Yate	94
——— Shunnor Fell	96
Mean	39095

Pickington Ridge to Pinch Yate.

By Pinch Yate and Water Crag	27534
——— Bakestone Edge	36
——— Shunnor Fell	40
Mean	27537

Pickington Ridge to Bakeston Edge.

By Bakest. Edge and Water Crag	22986
——— Pinch Yate	87
Mean	22986

Calvey to Bakestone Edge.

By B. Edge & Pickington Ridge	34412
——— Pinch Yate	30
Mean	34424

Calvey to Pinch Yate.

By Pinch Y. & Pickington Ridge	16222
——— Bakestone Edge	32
Mean	16226

Calvey to Pickington Ridge.

By P. Ridge & Bakestone Edge	19459
——— Pinch Yate	75
Mean	19470

Harker to Shunnor Fell.

	Feet.
By Shunnor Fell & Pinch Yate	55836
——— Bakest. Edge	781
Mean	55825

Harker to Bakestone Edge.

By Bakest. Edge & Shunnor Fell	31987
——— Calvey	32021
——— Pinch Yate	32038
Mean	32027

Harker to Pinch Yate.

By Pinch Yate & Calvey	23883
——— Shunnor Fell	913
——— Bakestone Edge	909
Mean	23901

Harker to Calvey.

By Calvey and Pinch Yate	10070
——— Bakestone Edge	87
Mean	10081

Nine Standards Hill to Water Crag.

By Water Crag & Shunnor Fell	34335
——— Bakestone Edge	28
Mean	34331

Nine Standards Hill to Shunnor Fell.

By Shun. Fell & Bakestone Edge	29896
——— Pickington Ridge	900
——— Water Crag	907
Mean	29901

Nine Standards Hill to Bakestone Edge.

By Bakest. Edge & Shunnor Fell	49541
——— Water Crag	50
——— Pickington Rid.	71
Mean	49549

Nine Standards Hill to Pickington Ridge.

By Picking. R. and Shunnor Fell	66994
——— BakestoneEd.	67012
Mean	66999

Blake Hill to Shunnor Fell.

By Shun. Fell & Bakestone Edge	27991
——— Water Crag	28001
——— Pickington Rid.	28002
Mean	27998

Blake Hill to Water Crag.

By Water Crag & Shunnor Fell	14820
——— Pinch Yate	19
——— Pickington Ridge	14
Mean	14818

Blake Hill to Bakestone Edge.

	Feet.
By Bakest. Edge & Shunnor Fell	17752
——— Pinch Yate	58
——— Pickington Ridge	63
Mean	17758

Blake Hill to Pickington Ridge.

By Pickington Ridge & Pinch Yate	27907
——— Shunnor Fell	09
——— Bakestone E.	10
——— Water Crag	10
Mean	27909

Blake Hill to Pinch Yate.

By Pinch Yate & Pickington Rid.	15884
——— Bakestone Edge	87
——— Water Crag	89
Mean	15887

Keasdon to Shunnor Fell.

By Shun. Fell & Bakestone Edge	18576
——— Nine Standards	83
Mean	18580

Keasdon to Nine Standards Hill.

By N. S. H. & Bakestone Edge	31666
——— Shunnor Fell	84
Mean	31680

Keasdon to Blake Hill.

By Blake Hill & Bakestone Edge	9905

Keasdon to Bakestone Edge.

By Bakest. Edge & Shunnor Fell	18839
——— Blake Hill	43
——— Nine Stand. Hill	60
Mean	18843

Hugh Seat to Shunnor Fell.

By Shun. Fell & Pickington R.	20064
——— Keasdon	71
——— Nine Stand. Hill	75
——— Blake Hill	76
——— Ingleborough	94
——— Water Crag	94
Mean	20079

Hugh Seat to Bakestone Edge.

By Bakest. Edge & Inglebro'	45571
——— Keasdon	571
——— Nine Stand. H.	575
——— Water Crag	601
——— Blake Hill	609
Mean	45585

Hugh Seat to Water Crag.

	Feet.
By Water Crag & Shunnor Fell	43086
——— Bakestone Edge	089
——— Blake Hill	088
——— Pickington Ridge	108
Mean	43093

Hugh Seat to Nine Standards Hill.

By Nine S. Hill & Shunnor Fell	17622
——— Bakestone Edge	26
Mean	17624

Hugh Seat to Keasdon.

By Keasdon and Bakestone Edge	32145
——— Shunnor Fell	48
Mean	32146

Hugh Seat to Knoutberry Hill.

By Calculation.................... 46476

Hugh Seat to Pen-y-gent.

By Calculation.................... 92264

Hugh Seat to Dod Fell.

By Calculation.................... 56606

The Tail Brigg to Shunnor Fell.

By Shun. Fell & Bakestone Edge	30452
——— Nine Stand. Hill	78
Mean	30465

The Tail Brigg to Bakestone Edge.

By Bakest. Ed. and Shunnor Fell	53043
——— Nine Stand. Hill	70
Mean	53056

The Tail Brigg to Nine Standards Hill.

By N. S. H. and Bakestone Edge	7721
——— Shunnor Fell	24
Mean	7723

Highest Standard to Water Crag.

By Water Crag and Blake Hill	34808
——— Pickington Ridge	10
Mean	34809

Highest Standard to Nine Standards Hill.

By N. S. H. and Pickington Ridge	1442
——— Water Crag	51
Mean	1447

East

East Stonesdale Moor to Shunnor Fell.

		Feet.
By Shun. Fell and Bakestone E.		27458
——————	Keasdon	64
——————	Nine Stand. Hill	67
	Mean	27463

East Stonesdale Moor to Nine Standards Hill.

By N. S. H. and Shunnor Fell		25579
——————	Bakestone Edge	79
	Mean	25579

East Stonesdale Moor to Bakestone Edge.

By B. Edge and Shunnor Fell		31132
——————	Nine Stand. Hill	44
	Mean	31138

East Stonesdale Moor to Keasdon.

By Keasdon and Shunnor Fell 13215

Dod End to Shunnor Fell.

By Sh. Fell and Pickington Ridge 23205

Dod End to Pickington Ridge.

By P. Ridge and Shunnor Fell		30547
——————	Blake Hill	49
	Mean	30548

Dod End to Keasdon.

By Keasdon and Blake Hill 5469

Rogan's Seat to Shunnor Fell.

By Sh. Fell and Bakestone Edge		29917
——————	Pinch Yate	33
——————	Pickington Ridge	34
	Mean	29928

Rogan's Seat to Water Crag.

By W. Crag and Pickington Ridge		6043
——————	Bakestone Edge	46
——————	Pinch Yate	46
	Mean	6045

Rogan's Seat to Pinch Yate.

By Pinch Yate and Shunnor Fell		16692
——————	Pickington Rid.	93
——————	Bakestone Edge	96
——————	Water Crag	97
	Mean	16695

Rogan's Seat to Pickington Ridge.

By P. Ridge and Pinch Yate		36977
——————	Water Crag	79
——————	Shunnor Fell	81
——————	Bakestone Edge	90
	Mean	36982

Brownsey to Shunnor Fell.

		Feet.
By Shun. Fell and Bakestone Ed.		35925
——————	Pickington Ridge	35
——————	Water Crag	36
	Mean	35932

Brownsey to Water Crag.

By Water Crag and Shunnor Fell		14847
——————	Bakestone Edge	52
——————	Pinch Yate	54
	Mean	14851

Brownsey to Pinch Yate.

By P. Yate and Pickington Ridge		8600
——————	Calvey	8600
——————	Water Crag	8601
	Mean	8600

Brownsey to Pickington Ridge.

By Picking. Ridge and Calvey		24256
——————	Pinch Yate	60
——————	Bakest. Edge	63
——————	Shunnor Fell	67
	Mean	24262

Satron Hangers to Shunnor Fell.

By Shun. Fell and Bakest. Edge		35102
——————	Water Crag	11
——————	Pinch Yate	14
——————	Blake Hill	13
	Mean	35110

Satron Hangers to Water Crag.

By Water Crag and Pick. Ridge		28920
——————	Shunnor Fell	22
——————	Bakestone Edge	26
——————	Pinch Yate	36
	Mean	28926

Satron Hangers to Bakestone Edge.

By Bake. Edge and Shunnor Fell		11122
——————	Water Crag	27
——————	Blake Hill	28
——————	Pickington Rid.	32
——————	Pinch Yate	32
	Mean	11128

Satron Hangers to Pickington Ridge.

By P. Ridge and Bakestone Edge		13911
——————	Pinch Yate	14
——————	Water Crag	19
——————	Calvey	25
	Mean	13917

Satron Hangers to Pinch Yate.

	Feet.
By Pinch Yate and Calvey	21874
——— Bakestone Edge	886
——— Pickington Rid.	887
——— Shunnor Fell	890
——— Blake Hill	890
——— Harker	890
——— Water Crag	900
Mean	21888

Holgate Pasture to Pinch Yate.

By Pinch Yate and Harker	32664
——— Bakestone Edge	68
Mean	32666

Holgate Pasture to Bakestone Edge.

By Bakestone Edge and Harker	57254
——— Pinch Yate	88
Mean	57271

Holgate Pasture to Harker.

By Harker and Bakestone Edge	29209
——— Pinch Yate	32
Mean	29220

The Hoove to Water Crag.

By Water Crag & Picking. Ridge	25048
——— Pinch Yate	54
Mean	25051

The Hoove to Pinch Yate.

By Pinch Yate and Picking. Ridge	17326
——— Calvey	33
——— Water Crag	36
——— Harker	38
Mean	17333

The Hoove to Harker.

By Harker and Pinch Yate	32106

The Hoove to Pickington Ridge.

By Picking. Ridge and Pinch Yate	40237
——— Water Crag	43
Mean	40240

Robincross Hill to Pickington Ridge.

By Picking. Ridge and Calvey	23451
——— Pinch Yate	54
——— Water Crag	57
Mean	23454

Robincross Hill to Pinch Yate.

	Feet.
By Pinch Yate and Pickington R.	39358

Robincross Hill to Water Crag.

By Water Crag and Picking. R.	53988

Whitfield Hill to Pickington Ridge.

By Picking. Ridge & Calvey	29972
——— Pinch Yate	29985
——— Water Crag	30000
Mean	29986

Whitfield Hill to Water Crag.

By Water Crag & Picking. Ridge	61165

Gibbon Hill to Pickington Ridge.

By Picking. Ridge and Water Crag	7488
——— Pinch Yate	9
——— Calvey	7
Mean	7488

Gibbon Hill to Pinch Yate.

By Pinch Yate and Pickington R.	25870
——— Calvey	76
Mean	25873

Gibbon Hill to Water Crag.

By Water Crag & Picking. Ridge	39260

Grinton Grits to Pickington Ridge.

By Picking. Ridge & Pinch Yate	11520
——— Calvey	20
Mean	11520

Grinton Grits to Pinch Yate.

By Pinch Yate & Picking. Ridge	29891
——— Calvey	905
Mean	29898

Nine Standards Hill to Pillar Hill.

By Calculation.....................	23568

[To be continued.]

II. *On the Obliquity of the Ecliptic.* *By* WILLIAM GALBRAITH, *Esq. M.A.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

THE subject of the obliquity of the ecliptic has so frequently engaged the attention of astronomers, that it may at first appear superfluous for me to attempt, in the present short sketch, to throw much light upon this interesting subject.

Still, however, it appears to me that sufficient attention has not been bestowed, at certain public institutions, on some parts of it, particularly that arising from a small error in the latitude, which, as will presently appear, produces a double effect on the difference between the summer and winter obliquity, by this means rendering that small error very sensible.

Let ω be the summer obliquity, ω' the winter, λ the latitude, z the zenith distance in summer, z' that in winter, and ε the error in latitude; then it will readily appear that in

Summer..... $\omega = \lambda + \varepsilon - z$ .... (1)
Winter...... $\omega' = z' - (\lambda + \varepsilon) = z' - \lambda - \varepsilon$ .... (2)
Whence $\omega - \omega' = 2\lambda - (z + z') + 2\varepsilon = \{2\lambda - (z + z') + 2\varepsilon\}$ ... (3)

But $2\lambda - (z + z')$ should be $= 0$, if the summer and winter obliquities are the same, as it is probable they are; whence

$$\omega - \omega' = \Delta\omega = 2\varepsilon \quad \ldots\ldots\ldots (4)^*$$

Or the difference of the summer and winter obliquity is affected by the sum of the errors in the latitude arising from using an erroneous table of refractions, a constant error in the instrument, &c.

Hence it is clear that an error in this determination will continue so long as the latitude and the obliquity of the ecliptic continue to be determined by Bradley's Refractions, which, though they possessed great merit at the time of their first appearance, and in a considerable degree still do so, yet are undoubtedly inferior to those called the French Refractions, as well as to those of Bessel, Brinkley, Ivory, and Young.

It has been long known that the Greenwich published determination of the obliquity in summer, does not accord with

* Indeed, without any attempt at demonstration, a very little consideration will show that the difference between the summer and winter obliquity is increased by twice the error of the latitude of the place of observation; since the zenith distance must be subtracted from the latitude in the former case, and the latitude must be subtracted from the zenith distance in the latter.

that

that obtained in winter, as may be seen in a table (20) published by Dr. Pearson in the first volume of his Treatise on Practical Astronomy, page 175; and that the winter obliquity is in general less than the summer, which seems to have been increasing in amount since the year 1754, in the time of Dr. Bradley. In the first volume of Dr. Maskelyne's Observations, page v. of his explanation of the tables, he expressly states that Bradley adopted $51° 28' 39\frac{1}{2}''$ N. as the latitude of Greenwich, which he was induced to increase by half a second, making it finally $51° 28' 40''$; and it is believed that this value of it continued to be used till his death, and perhaps to a still later period. From some calculations made from the Greenwich Observations by the French Refractions, I was induced to think the latitude about $51° 28' 38\frac{1}{2}''$, or exactly a second less than Bradley, and a second and a half less than Maskelyne.

Bessel has also investigated this matter in Schumacher's *Astronomische Nachrichten*, No. 73, apparently with great care, and obtains $51° 28' 38''\cdot34$, affected with a very small error, which cannot by the Greenwich Observations be removed, since the mural circle is incapable of reversion.

Again, in the Greenwich Observations for 1826, the latitude is deduced from numerous observations connected by various tables of refraction. But if a star near the zenith, such as γ Draconis, be chosen as least affected by any small error in the refractions, and a mean of the refractions in highest esteem be employed, as those of the French, Bessel, Brinkley, Young and Ivory, the latitude will be $51° 28' 38''\cdot5$, from which all those derived from tables of most authority deviate very slightly. This result may therefore, I presume, be confidently trusted as the true latitude; from which future observations will, in all probability, make no sensible deviation. The difference between the most common estimation of it, or $51° 28' 40''$, and $51° 28' 38''\cdot5$, is $1''\cdot5 = \text{E.}$, and therefore $2\epsilon = 3''$ will be the difference between the summer and winter obliquity arising from this cause alone; that is, the winter obliquity will be $3''$ less than the summer.

It will next be necessary to inquire what effect the refractions will have on the zenith distance in winter as being the greatest, and consequently most affecting, when taken from different tables. The meridian zenith distance of the sun at Greenwich near the winter solstice will be about 75°, as extreme nicety is not necessary to form an estimate in this case, at which few tables, especially Bradley's, are without the suspicion of error. For this purpose, suppose the atmosphere is

is in its mean state, or the barometer at 30 inches, and Fahrenheit's thermometer at 50°, in which case at 75°, Z. D., the

	′ ″
French tables will give the refraction =	3 34·90
Bessel's	3 34·30
Brinkley's	3 34·80
Young's (like Bessel's)	3 34·30
Ivory's	3 34·70
Mean of the whole	3 34·60
Bradley's	3 33·04
Probable error of Bradley's	— 1·56

That is, the correction to be added to the zenith distance is too small by 1″·56 in winter, and consequently the obliquity is also too small by the same quantity, on account of the error in Bradley's Refractions. This would, however, be partly corrected by the proportional defect at the summer solstice, which amounts to —0″·34; consequently the whole difference arising from this cause would be the difference between 1″·56 and 0″·34, or 1″·22 to be added to 3″·00 formerly obtained, making 4″·22 on the whole.

It may therefore, I think, be reasonably concluded, that when the latitude of Greenwich is assumed at 51° 28′ 40″, and the observed zenith distances of the sun at the solstices is corrected by Bradley's table of Refractions, the difference between the summer and winter obliquity will, independent of errors of observation, amount generally to 4″·22.

Now it will appear from an examination of the Greenwich determinations from about the year 1765 till 1790, in Dr. Pearson's table formerly alluded to, that the mean difference during that period is very nearly equal to that which has now been found. From 1790 till 1811 the differences are so great (about 10″ or 12″) that there must of necessity be some other error mixed up along with it. Perhaps the error of the mural quadrant then used was the principal cause.

Mr. Pond's observations from 1812 till 1819 inclusive, give a mean of —4″·87, so nearly the same as —4″·22, the quantity obtained on our theory, as to be a strong confirmation of its justness.

It is also confirmed by the observations of M. Bessel, whose results may be seen in table 4, page 436, of Dr. Pearson's work already referred to.

I am aware, however, that M. Cacciatore, superintendent of the Observatory of Palermo, has advanced a theory of this discrepancy of the obliquity, as determined by observations at the summer and winter solstices, depending on the effects of the

the heat of the sun upon his circle. Still, however, by means of equations, which give the necessary corrections attributable to this cause, he renders them nearly equal. It is generally understood that the Palermo circle made by the late Mr. Ramsden is very liable to be acted upon by heat during the time of observation, while that of Greenwich is much less so, or not very sensibly.

No doubt this may be one of the causes of discrepancy in others, and almost the sole cause in his, since it is very liable to alter its form by heat; but in more northerly latitudes it perhaps may be doubted whether this is the true cause, more especially as an adequate one, depending upon a different principle, has just now been produced.

I am, Gentlemen, yours &c.

Edinburgh, April 24, 1830. WILLIAM GALBRAITH.

III. *On the Vegetable Fossils found at Lennel Braes, near Coldstream, upon the Banks of the River Tweed in Berwickshire. By* HENRY WITHAM, *of Lartington, F.G.S. &c.**

HAVING been requested by several gentlemen to present to the public a statement of facts, respecting the fossil Flora in the neighbourhood of Coldstream, upon the banks of the river Tweed, I feel confident you will allow this paper a place in your valuable publication.

As the fossil vegetable kingdom has been divided into four great groups or periods, and as M. Adolphe Brongniart is of opinion that the transition is abrupt, and that there is a sudden difference in the most important characters of the vegetables, it becomes necessary for me to divide this memoir under three heads.

1st. A description of the surrounding strata.

2ndly. A description of the fossil plants themselves, with their position; and,

3rdly. A few observations upon both these heads.

1st. It has long been matter of dispute under what class of rocks the deposits in the neighbourhood of Coldstream are to be described. Some are of opinion that they are members of the old red sandstone series; and others, that they are to be classed with a much more recent deposit, the new red sandstone.

The following facts, which have been taken with great accuracy by my intelligent friend Mr. Francis Forster and myself, will I trust set this question at rest.—Immediately below the bridge at Coldstream, at its south end, you perceive a bed

* Communicated by the Author.

of

of gritty shale, belonging to the coal formation. It may be seen rising to the north-west at an angle of about 14°.

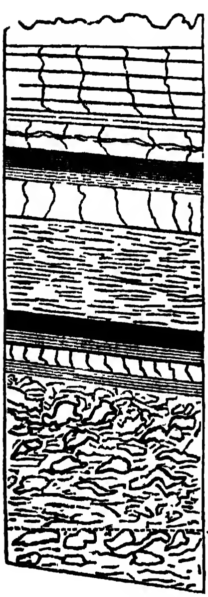
Above the bridge, on the north side, beds of sandstone, bituminous shale and iron stone, form the cliff rising 8° to the west.

Between the bridge at Coldstream and Lennel Braes, a distance of rather more than two miles, a great variety of shale and grit beds, evidently belonging to the coal formation, may be seen rising to the south and south-south-west; but irregular in their inclinations.

Lennel Braes being the place most exposed to the swelling waters of the Tweed, these ancient fossils are to be obtained there in the greatest abundance. It becomes necessary to be most minute; and I shall therefore present the following section.

Line of Section N. 15° E. dip in that direction 8°.

Scale 1 2 3 4 5 ft.



No.		Ft.	In.
10.	Sandstone (strong)	8	0
9.	Shale with two-inch bed of iron-stone	1	6
8.	Sandstone divided by shale	2	0
7.	Shale	4	0
6.	Sandstone	1	0
5.	Shale with three, six, and eight inch balls of ironstone and siliceous matter	9	0
4.	Shale	7	0
3.	Sandstone	1	6
2.	Sandstone in thin plates	2	0
1.	Shale confused in its beds (resembling the sill of a coal seam) containing the vegetable organic remains with their irregular layers of coaly matter	8	0
	These waved lines represent the river Tweed.		
	Feet	44	0

Both banks of the river are occupied by beds similar to the above, from Lennel Braes down to a very fine cliff about three-quarters of a mile above Sir David Milne's house.

This sandstone has many characteristic marks of the new

red sandstone formation. It is about forty-five feet in height, and nearly one hundred and twenty yards in length. This sandstone dips at an angle of 9° in the direction of north, 35° east. It is underlaid at its western point by a bed of shale containing organic remains of vegetables, and dipping north 73° east, at an angle of 16°. Hence it appears extremely probable that this is a lower bed of new red sandstone reposing on the coal measures.

On the south side of the river, in Twizell grounds, a similar sandstone is quarried. The position of this sandstone is altered near the edge of the bank, being bent down towards the river. It dips here 8° to the north-east, but in the western part of the quarry it appears to take a more regular course, and dips to the south at the rate of 8°. Further down the river a bed of shale is seen abutting against this sandstone, evidently thrown up by a fault. This quarry possesses all the characters of the new red sandstone.

There is also a bed of sandstone quarried in the burnside near Milne Graden, colour gray, close-grained, making a very fine building-stone. It dips at an angle of 5° to the south-south-east, and is probably a member of the new red sandstone.

Again:—A few hundred yards to the north of Coldstream is a thick bed of very fine sandstone belonging to the coal formation, dipping to the east-south-east, and, in its lower part, vegetable remains, mineralized by sulphate of iron, are lying in a horizontal position. It is underlaid by a bed of soft bituminous shale of unknown thickness, and the beds of sandstone are streaked and irregularly marked with the same substance.

These are all the remarks I think it necessary to make respecting the immediate rocks in which these singular fossils are to be found.

I beg leave, however, to mention that my intelligent young friend Mr. Charlton, jun. of Hesleyside, Northumberland, a member of the Natural History Society of Newcastle-upon-Tyne, accompanied us; and from the acute, and consequently striking observations made by him, I have little doubt that the world some day or other will be much instructed by such geological remarks as he may choose to submit to the public. Many people having great doubts how far the coal-field, which is worked to the south of the river Tweed, extended in a northerly direction, Mr. Forster and I proceeded from Coldstream to Greenlaw, between which places an extended and undulating plain of diluvium evidently covers the bassets of many strata.

On

On the top of the hill, south-east of Greenlaw, is a quarry of new red sandstone. It dips to the south at an angle of 19°. This great inclination may probably be attributed to the immediate vicinity of a whin dyke running a few yards to the north. In the direction of south 65° east (about four or five hundred yards west) this dyke has been worked to a width of twenty-seven yards. Whether this is its regular thickness, or an overflowing at this point, could not be ascertained, as the quarry does not extend more than four or five feet below the surface. The sandstone in the eastern quarry shows igneous phænomena in a striking degree. Another quarry of very red sandstone is worked on the side of the hill near Greenlaw. Between the bridge over the Blackadder, at the west end of Greenlaw, a bed of red sandstone, of twelve feet in thickness, is seen rising 14° in the direction of north 65° east.

The walls of Polworth are built of new red sandstone conglomerate from Leases quarry, about three-quarters of a mile to the west of this village.

Immediately below the bridge crossing Langton Burn, on the road from Polworth to Dunce, a bed of yellow sandstone, four feet in thickness, is underlaid by a bed of shale five feet thick, containing layers of iron-stone from six to eight inches in thickness, all rising about 22° to the north. It appears more than probable that there are members of the coal formation cropping from under the new red sandstone, which forms the hills behind them to the south.

At St. Helens, two miles south-east from Dunce, beds of shivery coal sandstone and shale, nearly horizontal, containing nodules of ironstone, occur. The coal shale also occurs extensively on the banks of the Blackadder, twelve miles from Berwick on the Paxton road, and repeatedly to the east on the banks of the said river.

At the bridge of the Whiteadder, one mile west of Churnside, a thick series of coal-measures dip rapidly to the east, forming a bold cliff, capped on the north side of the road by a thin detached bed much resembling new red sandstone.

Many interesting remarks might here be made, but I reserve them for a future paper, upon the red sandstones of Berwickshire.

2dly. The great abundance of these fossil plants in the above-named stratum lying in a state of much confusion, must be matter of surprise to those who have paid any attention to the ancient vegetation of the coal-fields in the north of England and Scotland. In all these fields it is well known the vascular cryptogamic plants appear greatly to prevail: and we have but occasionally been amused by some unheard-of

recumbent fossil, whose class, genus, or species generally occasioned much comment, and not a little hesitation.

Since the introduction of the art of slicing the stems of these fossil plants, the difficulty, which before appeared almost insurmountable, has been greatly removed; and I take much pride and pleasure in having recommended this method to the York and Newcastle Philosophical and Natural History Societies. The method is a beautiful one; and I think I do not exaggerate in saying, that by it we are enabled at once to observe the internal structure of any monocotyledonous or dicotyledonous plant. It is a refinement in the art which opens to view that which, from its antiquity and opacity, was before hidden from the sight of man. It enables us to view those early productions which for thousands of years have (when by accident exposed) either been neglected, or represented as something monstrous and absurd.

In this position, amongst the members of the independent coal-field, however, we have every reason to believe in a forest of unknown extent, all apparently of the same genus, differing altogether from the vascular cryptogamic plants.

To what genus and species are we therefore to refer these ancient remains of a former world?

They cannot be vascular cryptogamic plants, as they contain decided woody texture from the centre of the stem.

They cannot be monocotyledonous, the pith not forming the greater part of the stem, and the woody parts not being composed of fasciculi, which are disseminated throughout the pithy texture of plants of that kind.

They having, in my opinion, most decided medullary rays, it would therefore appear to me that they must be classed amongst the dicotyledonous plants.

As such, therefore, after repeated and most minute microscopic examinations and comparisons, not only with fossil but with recent plants, I do not hesitate to class these numerous fossil vegetable remains.

Lastly:—By the above observations it appears therefore quite clear, that the coal-measures to the south of the river Tweed by no means terminate at or near the ancient boundary of the two kingdoms, but approach within a short distance of the transition range of the south of Scotland.

The contorted and flattened shape of many of these ancient stems is worthy of remark. Their external coatings are invariably carbonized: probably their present forms may have been caused by extreme pressure when these vegetables were in a state of decomposition; and subsequently it was, that foreign substances, by percolation, took possession of the decayed

cayed portions of the plants. It is difficult also to ascertain their height, as by the above description they must have been liable to be fractured and broken. The highest stem I have been able to obtain is not much more than four feet, and the lower part of it is about six feet in circumference. No two stems possess the woody appearances alike, some retaining it in the centre of the stem, others having such appearances distributed in various parts of the stem. Owing to the immense superincumbent mass, this part of the research is rendered both tedious and expensive.

By these examinations it is equally evident, that this unknown extent of early vegetation seems to have been called into existence during the formation of the carboniferous rocks, or in the first period of Brongniart's division. Now, according to that gentleman's opinion, out of six classes (with the exception of the marine and a few uncertain plants) only two existed at that period; namely, the vascular cryptogamic plants, comprehending the *Filices*, *Equisetaceæ*, *Lycopodiæ*, and the monocotyledons, containing a small number of plants which appear to resemble the palms and arborescent Liliaceæ; in fact, Mr. A. Brongniart states, that out of 260 species discovered in this *terrain*, 220 belong to the vascular cryptogamic group.

The existence, therefore, of so extensive a deposit of dicotyledonous plants at this early period of the earth's vegetation appears to demand the attention of the naturalist; and it does indeed go far to prove the necessity of more minute examination amongst the dark and pathless repositories of an antediluvian world.

The following is the analysis of one of these plants:

Twenty grains of Tweed fossil yields

Carbonate of lime	16·65
Carbon	3·30
Iron (peroxide)	0·68
Loss	0·37
	20·00

The similarity of the above analysis to that of the fossil stem found in Craigleith quarry, in the year 1826, is very remarkable*. From recent minute examination and comparisons, there is reason to believe that plant to be a member of the same class as the above-described ancient fossils.

Edinburgh, May 10, 1830.

* See Phil. Mag. and Annals, N.S. vol. vii. p. 29.—Edit.

IV. *On*

IV. *On the Power of Horses.* *By* B. Bevan, *Esq.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

TO determine the average power of horses under different kinds of labour, has been a subject deemed worthy of the inquiries of many of the first class of scientific writers. It is one of those points which can be determined only by experiment. The power to be maintained depends upon the velocity; and various formulæ are given by writers on this subject. Thus, Professor Leslie gives $(15-v)^2$ = pounds avoirdupoise for the power of traction of a strong horse, and $(12-v)^2$ = pounds traction of the ordinary horse, v = velocity in miles per hour.

In the period from 1803 to 1809, I had the opportunity of ascertaining correctly the mean force exerted by good horses in drawing the plough; having had the superintendence of the experiments on that head at the various ploughing matches, both at Woburn and Ashridge, under the patronage of the Duke of Bedford and the Earl of Bridgewater. I find among my memoranda the result of eight ploughing matches, at which there were seldom fewer than seven teams as competitors for the various prizes.

		lbs.
The 1st result is from the mean force of each horse in six teams of two horses, each team upon light sandy soil	=	156
The 2nd result is from seven teams of two horses, each team upon loamy ground, near Great Berkhamstead	=	154
The 3rd result is from six teams of four horses, each team with old Hertfordshire ploughs	=	127
The 4th result is from seven teams of four horses, each team upon strong stony land (improved ploughs)	=	167
The 5th result is from seven teams of four horses, each team upon strong stony land (old Hertfordshire ploughs)	=	193
The 6th result is from seven teams of two horses, each team upon light loam	=	177
The 7th result is from five teams of two horses, each upon light sandy land	=	170
The 8th result is from seven teams of two horses, each team upon sandy land	=	160

The mean force exerted by each horse from fifty-two teams, or 144 horses, = 163 pounds each horse; and although the speed

speed was not particularly entered, it could not be less than at the rate of 2½ miles per hour.

As these experiments were fairly made, and by horses of the common breed used by farmers, and upon ploughs from various counties, these numbers may be considered as a pretty accurate measure of the force actually exerted by horses at plough, and which they are able to do without injury for many weeks;—but it should be remembered, that if these horses had been put out of their *usual* walking pace, the result would have been very different. The mean power of the draught-horse, deduced from the above-mentioned experiments, exceeds the calculated power from the highest formula of Mr. Leslie. I am, Gentlemen,

Your obedient servant,

B. Bevan.

V. *Narrative of an Excursion to the Summit of the Peak of Teneriffe on the 23rd and 24th of February* 1829. *By* Robert Edward Alison, *Esq.**

HAVING frequently observed the rapid fall of the atmospheric temperature when ascending from the sea-coast of Teneriffe to an elevation of 1000 feet, and on the contrary the very slow alteration at higher points, I was desirous of ascertaining the proportional decrement of heat in the upper regions.

The ascent to the top of the Peak of Teneriffe is considered by the natives of the island as impracticable in the winter season, on account of the snow and the supposed extreme cold; it is therefore seldom made before the month of June, when it is free from snow: but I was not able to wait for a more genial season, as I was in daily expectation of returning to England; I therefore resolved to make the attempt.

On the 23rd of February, at 4 A.M., I left the Augustine convent Orotuca, which is situated nearly 1100 feet above the level of the sea. At the time of my departure the thermometer stood at 56°·5, and there was a gentle breeze from the N.N.W.

After joining a stout active young peasant who was to act as my guide, we entered the "Camino de Crasna," which is dignified by the name of a road, although, like almost all the highways in the island, it is only a steep and uneven line of

* Communicated by the Author.

lava

lava. An ascent of 500 feet took us to the extremity of the beautiful zone of vines, and at half-past six A.M. we crossed the barranco or ravine Penilla; the surface here gradually began to assume a different appearance, from the vegetation which clothed it being common to Europe.

Shortly after leaving the zone of chestnuts and lupines, we entered a belt of heaths which form the third region of plants; this belt is about four miles long, and a quarter of a mile broad, and extends nearly across the valley of Orotava. The temperature here began to decrease very sensibly; and although the lavas in some places were only covered with a few inches of decomposed volcanic and vegetable matter, yet they were clothed with a most luxuriant verdure. This is no doubt to be attributed to the moisture from the clouds, which frequently completely envelop even this inferior elevation towards the close of the day, or before or after any sudden alteration of temperature.

Fifteen minutes after leaving the last barranco, we crossed another, called Pilloni, which is rather more than 3000 feet above the sea; and soon after, we entered the Barranco del Pino Dornajito, which is 3410 feet above the sea*; it is so named from an enormous pine-tree that grew near the western side of the ravine. It is said that this tree was full-grown at the time of the conquest of the island, 360 years ago;—thus, having stood the storms of so many ages, it was at last swept into the ravine by the dreadful waterspout that devastated the island on the 7th of November 1826. Although this tree is partly destroyed by its fall, yet it still measures 128 feet in length, and 30 in circumference.

Under a precipice in the middle of the ravine is a small spring of water, with a wooden cross at the side of it; the temperature of the spring was 56°, but it appears to vary more than any other which I have examined, as in October the temperature of the water was 65°·5. At the time of the before-mentioned waterspout, a body of water, some hundred feet wide and thirty or forty deep, fell over this spring and cross without doing it the least damage; which the peasantry attribute to the Divine interposition, forgetting that the water, in falling from the height above, would form a curve and effectually protect it from injury.

From the great depth of this ravine, the various strata of lava can be observed; the superior stratum consists of decomposed

* According to a barometrical admeasurement made by Humboldt and calculated by the formula of Laplace.

posed lava and vegetable mould, to the depth of three feet; next is a sort of volcanic breccia or conglomerate, held together by a brown mud and tufa; afterwards a bed of volcanic tufa, of four or five feet in thickness, succeeded by alternate strata of compact bluish-coloured basaltic trap and brown mud. In the bed of the ravine were large blocks of lava mixed with hornblende and augite.

In my first ascent to the Peak I was much disappointed in not meeting with the forest of pine-trees above the zone of chestnuts, which the visitors to the Peak during the last century mention with so much pleasure; but the destructive axe has not left a single trace of them. On the way towards the defile of the Portillo, there are several spots pointed out as famous for the enormous size of a particular pine-tree, such as *Pino del Dornajito*, *Pino de la Caravela*, *Pino de la Merienda*, and *Ultimo Pino*.

A short distance above Dornajito the surface is intersected by numerous small ravines, and the soil is so thin that the lava frequently appears above the surface. Ascending a little further we approached the lower region of the clouds, where we found the temperature begin to fall rather rapidly; but at the same time vegetation became so luxuriant, that it was difficult to observe the nature of the lavas. The tree heaths (*Erica arborea*) are of considerable size, being sometimes sixteen or eighteen feet high, with stems half the thickness of a man's body; they are mixed with the laurel, cytisus, and various other arborescent shrubs: it is worthy of remark, that all the leaves of the different shrubs here are of the same size, form and colour, which is a brilliant dark green. This strong vegetation is certainly produced by the humidity of the atmosphere, from the clouds generally resting at this elevation during the night and early part of the morning.

What makes this more evident is, that as you advance to the verge of this climate, where the air is drier and the sun's rays more powerful, vegetation becomes less luxuriant, and the leaves are of a light green instead of a dark green colour.

The lavas here appeared to have flowed in numerous streams from various openings; they look like a sort of wacken: their colour is black, rather vesicular, and they contain green-coloured augite, hornblende, and olivine, and all affect the magnetic needle strongly. Many of the lavas are much decomposed, with the pores on the outside free from crystals, which have no doubt fallen out, as I always found them internally.

We still gradually ascended; and as we crossed Barranco Haya we were rather annoyed by the lower region of clouds condensing on our clothes and bodies, and producing by the evaporation a degree of cold considerably greater than that indicated by the thermometer, which was 49·5; we likewise encountered a strong current of air blowing from the west-by-south, although both above and below us the wind was blowing from the N.N.W. This current of air was produced probably by the wind sweeping down the western extremity of the mountain called Tigayga, into the entrance of the Cañadas, between the Cavison and the Portillo (which is a sort of defile or opening in the chain of mountains that surround the Cañadas), thus creating a considerable pressure of air, which caused a rush of wind into the valley below.

Some distance above Barranco Haya we crossed an ancient stream of lava, which appeared to have proceeded from a volcano to the S.S.W., but it was so covered with vegetation that it was difficult to discover its composition. The detached pieces which I picked up were a porphyritic trachyte, with crystals of augite and felspar, partly destroyed by fire.

At ¼ to 8 A.M. we crossed the ravine named Fuera el Monte, and entered the *Llanos de Gaspar* (the plains of Gaspar). Here vegetation became very scanty, and almost the only plant was Canarian thyme. But this spot is particularly interesting, from its being evident that a considerable part of the waterspout which deluged the island in November 1826 had burst here, cutting the surface into a vast number of ravines, some of them of great depth. From the appearance of the surface, the columns of water which fell must have been very numerous; as in ten or twelve different places the lava is cut into deep trenches, some of them fifteen and twenty feet deep, with the soil which was between them completely washed away by the spray or overflowing of the water. Many of these deep channels frequently converge into one, forming a destructive and overwhelming ravine.

Considerable bodies of water have frequently fallen upon the Canaries, and done some mischief by washing into the sea the vegetable mould which so thinly covers the lavas. But the visitation of the 6th and 7th of November 1826 was the most awful and destructive, both to life and property, of any of which the inhabitants have any tradition. My friend Mr. Auber, of Orotava, has furnished me with the interesting details of the phænomena attending this waterspout, which I shall here subjoin.

On the afternoon of the 6th of November, the wind, which

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was blowing strongly from the N.E., veered round to every point of the compass, and ultimately established itself from the north; but at sea, a few miles from land, it was blowing a hurricane from the N.E., and in a moment, without any intermediate change, it blew as strongly from the S.W. The sky became obscured all at once by enormous masses of black clouds, which hastened the night some time before sunset; but neither thunder nor lightning was observed. The rain commenced to fall in torrents towards 10 o'clock at night, and the wind to blow with an overpowering impetuosity. At half-past two on the morning of the 7th, Mr. Auber observed several globes of fire moving upon the sea, at various distances from the shore, whilst others remained stationary. One of them, from its position, appeared to be on the top of the Montañeta of Realejo, and caused him to suppose that that extinct volcano was going to threaten the valley of Orotava with an eruption: but he was soon undeceived, by observing that the globe moved about on the surface of the water like the others, and at some distance from the spot where he first thought it was situated.

These luminous globes appeared to move towards the S.W. and follow the direction of the waves. The light which they spread in the atmosphere extended more than 45° high; and although he was three miles off, it was often sufficiently strong to enable him to read rather small print; but no detonation was heard. The number of globes increased from half-past two o'clock till four, when they began to diminish. Mr. Auber, at one period of his observations, counted fourteen moving about at one time; but the glare of light which he perceived on his right, where the surrounding houses bounded his view, caused him to suppose their number to be much more considerable. Their duration was from one minute to five or six, but seldom longer; and their apparent diameter was about the half of that of the moon at her full, when she reaches the zenith. When they had all disappeared the darkness was extreme, and he could not see the neighbouring houses; but a quarter of an hour afterwards, the reappearance of the same globes, or the formation of new ones, allowed him to see the island of Palma, though nearly sixty miles distant. The rain fell with equal force whilst these globes were appearing upon the sea and after their disappearance. It was mentioned that a globe of fire had fallen at the foot of the mountain of Tigayga, which bounds the valley of Orotava to the west, and that it had made a deep hole in the earth:—search was made respecting the truth of this assertion, but it did not lead

to any positive result. I was likewise informed that similar globes of fire were seen traversing the Llano de Gaspar, the spot which I have mentioned as bearing such evident marks of the effects of the water. My informant, who was a small farmer living near Tigayga, and almost on a level with the Llano de Gaspar, likewise added, "that all the heaths appeared to be on fire; and at the same time I saw a column of water several fathoms wide move across the top of the valley."

I will now resume the thread of my narrative to the Peak: and for the purpose of pointing out the devastation committed in 1826, I shall incur the risk of being thought tedious, by enumerating the ravines which I crossed at the spot where the waterspout appeared to have burst. The first was Barranco de Llano de Gaspar; it was of some depth, and exposed a stratum of basaltic lava, a species of puzzolana of considerable thickness, and a brown volcanic mud resting on a bed of close black lava. A little to the west were two new ravines, which united into one at a short distance from the commencement, and formed the barranco which did so much mischief to the port of Orotava. The next was a new ravine, and is only remarkable for being the spot where you take leave of the luxuriant vegetation of the third zone, and enter that of the cytisus, which may be termed the fourth zone of plants. The surface here is a brown volcanic mud mixed with small pieces of lava, forming a hard breccia or conglomerate, with a slight covering of vegetable mould, which in many places between the ravines was completely washed away by the spray of the water.

Towards the south-western extremity of the Llano de Gaspar is a spot named the Camina del Alta, where there is a stream of trachytic lava that has separated at a short distance above, and formed a sort of half-circle: the two streams are nearly destitute of vegetation. Another column of water appears to have burst here, and made three or four ravines, which converge into one a few hundred feet below. At 8·30 A.M. we entered a part of the inclined plane called Chasquitas Abaxo and Chasquitas Arriba. The ravines here are very numerous, and some are so close together that there is hardly space sufficient to pass between them. Within a few hundred yards I crossed eleven, which were all formed in 1826; and in the upper part of Chasquitas Arriba the surface was cut into almost innumerable trenches of various depths, according to the force of the water or the compactness of the lava.

When we gained the top of a rather steep acclivity called Lomo

Lomo de la Calavera, we met with a new barranco running into an ancient one of the same name as the hill; and about three-quarters of a mile from it we came to Barranco Juradillo, which is of an immense breadth and depth. At the spot where we crossed it the torrent had divided itself into two branches, forming a sort of islet in the centre. The sides of the ravine were composed of various strata of lava and mud: the superior stratum was basaltic trap, occasionally inclined to a columnar formation; the second was a brown volcanic mud, about ten feet thick, below which was trap in laminar masses, volcanic breccia, and a sort of colorific earth. A short distance beyond Juradillo we passed on our left hand a hill of pumice *, which had been cut down in a perpendicular manner to the depth of at least eighty feet by the waterspout of 1826.

The surface now entirely consisted of white rapilli partly decomposed, and masses of porphyritic lava, occasionally mixed with veins of pumice in the centre. Some time before we gained this elevation we found vegetation gradually becoming less luxuriant and more and more scanty, till here it was reduced to one variety only, the mountain broom (*Spartium nubigenum*), which is the last plant of the upper regions, and indicates the fifth and highest zone of plants. The dry, close, and ligneous formation of its leaves fully enables it to support the immense difference of temperature which it is obliged to undergo every four-and-twenty hours. During the summer season, in the day-time, the intensity of the solar rays is almost insupportable on account of the nature of the soil and the clearness and rarefaction of the air: on the contrary, the night air is excessively cold and moist. In winter the snow is permanent for some months, which, joined to the great elevation, produces a cold equal to that of the arctic regions.

At 10 P.M. we passed a spot called the *Ultimo Pino* (last pine), and had a view of the foot of the Peak, which bore W. by N. of us. The view on our right was a novelty to a person who was not accustomed to ascend great elevations. The valleys below were filled with vapours, whilst the sea and the regions above were quite clear. Objects below were unusually refracted. Two brigantines, which were just in the horizon, presented inverted images of some of their parts; but what was very singular, they occasionally altered their form: sometimes

* At this spot the waterspout brought to light two earthen bowls which had belonged to the Guanches. The possessor of them, Don Lorenzo Machado, very kindly presented me with one of them; it is made of argillaceous earth, mixed with a black volcanic sand, and is sun-dried: it holds about 200 cubic inches of water.

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the masts and ship appeared as if they were separated, then the masts touched each other, and afterwards rapidly increased in length, presenting quite a distorted appearance. The sea looked as if it were on a level with the eye; and although it was four or five miles off, the ripple upon the water was distinctly visible. This refraction was probably caused by the difference of temperature between the lower and upper stratum of air (18° of Fahrenheit), which produced a medium of varied density; and from the extreme evaporation over the sea, the refractive power there was small, but gradually increased to the point of observation, which possibly was its utmost limit, as after we ascended a few yards this unusual refraction or *mirage* went off, and the vessels assumed their usual appearance.

[To be continued.]

VI. *A direct Method of finding the shortest Distance between two Points on the Earth's Surface when their Geographical Position is given.* By James Ivory, *Esq. M.A. F.R.S. &c.**

THE geometers who, supposing that the earth is an oblate elliptical spheroid of revolution, have investigated rules for computing the shortest distance between two points on its surface, usually assume that there are given the two latitudes and the angle which the geodetical line makes with the meridian of one of the points. The direct problem for deducing the geodetical distance from the geographical position of the two points has not hitherto been solved. In the *Conn. des Tems* for 1832, M. Puissant has given formulæ which supply this defect. But his method merely consists in deriving from the two latitudes and the difference of longitude, what is necessary for applying the formula for the shortest distance published long ago by M. Legendre. I shall here shortly explain a different solution of the problem, which I lately obtained; according to which the shortest distance of two points on the earth's surface is expressed by means of their latitudes and the inclination to the equator of the great circle of the celestial sphere that passes through them.

The radius of the equator of an oblate elliptical spheroid of revolution being represented by unit, and the semipolar axis by $\sqrt{1-e^2}$, let λ denote the latitude of a point on the surface, and ψ the longitude reckoned from a fixed meridian: then, if x, y, z be the three coordinates of the point, the ori-

* Communicated by the Author.

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gin being in the centre, x being perpendicular to the equator, and y to the fixed meridian, we shall have, by the properties of the spheroid,

$$\Delta = \sqrt{1 - e^2 \sin^2 \lambda},$$

$$x = \frac{(1-e^2)\sin\lambda}{\Delta}, \quad y = \frac{\cos\lambda \sin\psi}{\Delta}, \quad z = \frac{\cos\lambda\cos\psi}{\Delta}.$$

Further, ds being the element of a curve on the surface of the spheroid, we shall have,

$$ds^2 = dx^2 + dy^2 + dz^2;$$

and if we substitute the differentials of the coordinates, there will result

$$ds^2 = \frac{(1-e^2)^2 d\lambda^2}{\Delta^6} + \frac{\cos^2\lambda d\psi^2}{\Delta^2}.$$

This expression is general, whatever be the nature of the curve. In order to find the equation of the geodetical line with the least calculation, I shall make ds and $d\psi$ only vary;

then, $$d\delta s = d\delta\psi . \frac{\cos^2\lambda d\psi}{\Delta^2 ds}:$$

consequently,

$$\delta s = \delta\psi . \frac{\cos^2\lambda d\psi}{\Delta^2 ds} - \int \delta\psi d . \frac{\cos^2\lambda d\psi}{\Delta^2 ds}.$$

The beginning and end of the curvilineal arc being fixed, the nature of the line of shortest distance requires that,

$$d . \frac{\cos^2\lambda d\psi}{\Delta^2 ds} = 0, \text{ and, } \frac{\cos^2\lambda d\psi}{\Delta^2 ds} = c,$$

c being a quantity that remains constant in the whole length of the curve.

Using now this last equation to exterminate $d\psi$ from the general expression of ds^2, we shall get,

$$ds\sqrt{1 - \frac{c^2\Delta^2}{\cos^2\lambda}} = \frac{(1-e^2)d\lambda}{\Delta^3}:$$

and, if we make,

$$b^2 = \frac{1-c^2}{1-e^2c^2},$$

we shall further obtain,

$$s = \sqrt{1-e^2} . \sqrt{1-e^2b^2} . \int \frac{d\lambda \cos\lambda}{\sqrt{b^2 - \sin^2\lambda}} . \frac{1}{\Delta^3}.$$

We likewise have,

$$\psi = c \times \int \frac{ds . \Delta^2}{\cos^2\lambda} = \sqrt{1-e^2} . \sqrt{1-b^2} . \int \frac{d\lambda}{\cos\lambda \sqrt{b^2 - \sin^2\lambda}} . \frac{1}{\Delta}.$$

In order to deduce the values of s and ψ from the foregoing expressions, it would seem to be necessary to integrate them between the initial and final latitudes λ' and λ, of which the

the first may be supposed the less. But we may proceed in a different manner. If we suppose that e^2 varies, while the extremities of the curvilineal arc continue upon the same meridians, and retain constantly the same latitudes, ψ will remain unchanged, but s will have different values as e^2 assumes different magnitudes. When $e^2 = 0$, s will become equal to an arc σ of a great circle on the surface of the circumscribing sphere, the extremities of σ having the same latitudes, and the same difference of longitude, as the two points on the surface of the spheroid; and at the same time b will become equal to β, the sine of the inclination of the same great circle to the equator. Now the values of s and b, when $e^2 = 0$, being σ and β, we may assume in general,

$$s = \sigma + A e^2 + B e^4 + \&c.;$$

then, $$\frac{ds}{e\,de} = 2\,A, \quad \frac{d\left(\frac{ds}{e\,de}\right)}{e\,de} = 8\,B, \&c.,$$

the values of the differentials, which must be deduced from the foregoing expressions of s and ψ, being estimated on the supposition that $e^2 = 0$.

For the sake of simplicity, let Q be written for $\sqrt{b^2 - \sin^2\lambda}$; then, by differentiating the two expressions of s and ψ with respect to e^2, ψ being constant and b^2 a function of e^2, we shall get,

$$-\frac{b\,db}{e\,de}\cdot\int\frac{d\lambda\cos\lambda}{Q^3}\cdot\frac{1}{\Delta} - \frac{1-b^2}{1-e^2}\int\frac{d\lambda\cos\lambda}{Q}\cdot\frac{1}{\Delta^3} = 0, \qquad (1)$$

$$\frac{ds}{e\,de} = -\frac{b\,db}{e\,de}\cdot\frac{\sqrt{1-e^2}}{\sqrt{1-e^2b^2}}\cdot\int\frac{d\lambda\cos\lambda}{Q^3}\cdot\frac{1}{\Delta}$$

$$-\frac{1+b^2-2e^2b^2}{\sqrt{1-e^2}\sqrt{1-e^2b^2}}\cdot\int\frac{d\lambda\cos\lambda}{Q}\cdot\frac{1}{\Delta^3}$$

$$+ 3\sqrt{1-e^2}\cdot\sqrt{1-e^2b^2}\cdot\int\frac{d\lambda\cos\lambda\sin^2\lambda}{Q}\cdot\frac{1}{\Delta^5}:$$

and by combining the two differential expressions so as to exterminate $\frac{b\,db}{e\,de}$ from the latter, there will result,

$$\frac{ds}{e\,de} = -2\,b^2\frac{\sqrt{1-e^2}}{\sqrt{1-e^2b^2}}\int\frac{d\lambda\cos\lambda}{Q}\cdot\frac{1}{\Delta^3}$$

$$+ 3\sqrt{1-e^2}\cdot\sqrt{1-e^2b^2}\cdot\int\frac{d\lambda\cos\lambda\sin^2\lambda}{Q}\cdot\frac{1}{\Delta^5}.$$

Further it will be found that

$$\sin^2\lambda = \frac{b^2\Delta^2 - Q^2}{1-e^2b^2};$$

and

and by substituting this value of $\sin^2 \lambda$ in the second term of the right side of the last equation, we shall get,

$$\frac{ds}{e\,d\,e} = \frac{\sqrt{1-e^2}}{\sqrt{1-e^2 b^2}} \times \left\{ b^2 \int \frac{d\lambda \cos\lambda}{Q} \cdot \frac{1}{\Delta^3} - 3 \int \frac{d\lambda \cos\lambda Q}{\Delta^5} \right\}. \quad (2)$$

If in this expression we make $e^2 = 0$, $b^2 = \beta^2$, we shall have,

$$\frac{ds}{ede} = 2\mathrm{A} = \beta^2 \int \frac{d\lambda \cos\lambda}{\sqrt{\beta^2 - \sin^2\lambda}} - 3 \int d\lambda \cos\lambda \sqrt{\beta^2 - \sin^2\lambda}.$$

In order to perform the integrations, put $\beta \sin a = \sin \lambda$, $\beta \sin a' = \sin \lambda'$; then

$$2\mathrm{A} = \beta^2 \int d\,a - 3\beta^2 \int d\,a \cos^2 a\,;$$

and, by integrating between the limits a' and a,

$$2\mathrm{A} = -\frac{\beta^2}{2}(a-a') - \frac{3}{2}\beta^2 \sin(a-a') \cos(a+a').$$

Now if the great circle of which σ is a part intersect the equator, a and a' are the arcs between the intersection and the extremities of σ: wherefore $a-a' = \sigma$, and

$$2\mathrm{A} = -\frac{\beta^2}{2}\sigma - \frac{3}{2}\beta^2 \sin\sigma \cos(a+a'). \quad (3)$$

In like manner if we make $e^2 = 0$, and $b^2 = \beta^2$, in the differential equation (1), we shall get,

$$-\frac{b\,d\,b}{e\,d\,e} \cdot \int \frac{d\lambda \cos\lambda}{(\beta^2 - \sin^2\lambda)^{\frac{3}{2}}} - (1-\beta^2) \int \frac{d\lambda \cos\lambda}{\sqrt{\beta^2 - \sin^2\lambda}} = 0\,;$$

which is transformed, by the same substitutions as before, into this which follows,

$$-\frac{b\,d\,b}{e\,d\,e} \cdot \frac{1}{\beta^2} \int \frac{d\,a}{\cos^2 a} - (1-\beta^2) \int d\,a = 0\,;$$

from which we readily deduce,

$$\frac{b\,d\,b}{e\,d\,e} = -\beta^2 (1-\beta^2) \frac{\sigma}{\sin\sigma} \cos a \cos a'. \quad (4)$$

In order to determine the other coefficient B, I shall write M for that part of equation (2) which is multiplied by the radical $\sqrt{\frac{1-e^2}{1-e^2 b^2}}$; and I shall expand the radical in a series, then

$$\frac{ds}{ede} = \mathrm{M} - \frac{e^2}{2}(1-b^2)\,\mathrm{M} - \&c.$$

By differentiating this equation and making $e^2 = 0$ after the operation,

$$\frac{d\left(\frac{ds}{ede}\right)}{ede} = 8\,\mathrm{B} = \frac{b\,d\,b}{e\,d\,e} \cdot \frac{d\,\mathrm{M}}{b\,d\,b} + \frac{d\,\mathrm{M}}{e\,d\,e} - (1-\beta^2)\,\mathrm{M}.$$

Now in this expression, the value of $\frac{b\,d\,b}{e\,d\,e}$ is known by the formula

formula (4); and M, equal to $\frac{ds}{e\,de} = 2A$, is known by the formula (3): it therefore only remains to compute the partial differentials of M, which is easily accomplished in the manner already explained. Omitting the detail of the calculation for the sake of brevity, the result will be as follows:

$$8\,B = \sigma\left(\frac{3}{2}\beta^2(1-\beta^2) - \frac{3}{2}\beta^4 + \beta^2\,(1-\beta^2)\,\frac{\sigma}{\sin\sigma}\cos a\cos a'\right)$$
$$+\left(\frac{3}{2}\beta^2(1-\beta^2) - \frac{3}{2}\beta^4\right)\sin\sigma\cos(a+a')$$
$$+\frac{15}{16}\beta^4\sin 2\sigma\cos 2(a+a').$$

The values of A and B being now found, we have only to substitute them in the assumed expression of s; but in doing this I shall write $\sin i$ for β, the symbol i standing for the inclination to the equator of the great circle of the celestial sphere, which passes through the two given points. Thus the following formula is obtained:

$$S = \sigma\,.\left\{1 - \frac{e^2}{4}\sin^2 i + \frac{3e^4}{16}\sin^2 i\cos^2 i - \frac{3e^4}{64}e^4\sin^4 i\right.$$
$$\left.+\frac{e^4}{8}\sin^2 i\cos^2 i\,\frac{\sigma}{\sin\sigma}\cos a\cos a'\right\}$$
$$-\left\{\frac{3e^2}{4}\sin^2 i - \frac{3e^4}{16}\sin^2 i\cos^2 i + \frac{3e^4}{16}\sin^4 i\right\}\sin\sigma\cos(a+a')$$
$$+\frac{15e^4}{128}\sin^4 i\sin 2\sigma\cos 2\,(a+a').$$

As this formula is perfectly general, it must comprehend the case when the two points are on a meridian, that is, when they are situated in a great circle perpendicular to the equator. On this supposition we have $\sin i = 1$, $\cos i = 0$, $a = \lambda$, $a' = \lambda'$, $\sigma = \lambda - \lambda'$; and the formula becomes,

$$S = (\lambda-\lambda')\,.\left(1 - \frac{e^2}{4} - \frac{3e^4}{64}\right)$$
$$-\left(\frac{3e^2}{4} + \frac{3e^4}{16}\right)\sin(\lambda-\lambda')\cos(a+a'),$$
$$+\frac{15e^4}{128}\sin 2\,(\lambda-\lambda')\cos 2\,(a+a'),$$

which coincides with the usual formula for the length of an arc on the meridian of an oblâte elliptical spheroid.

The same method of investigation which I have here used may be applied with advantage to other cases of spheroidical trigonometry, as I may show on another occasion.

June 14, 1830. JAMES IVORY.

VII. *Notes*

VII. *Notes on the Geographical Distribution of Organic Remains contained in the Oolitic Series of the Great London and Paris Basin, and in the same Series of the South of France.* *By* Henry T. De la Beche, *F.R.S. &c.*

[Continued from vol. vii. page 351.]

7. Gryphæa Macculochii (*Sow.*). *Lias.* Western Islands, Scotl. (Murch.). *Lias.* Yorks. (Phil.). *Oxford clay.* Norm. (De C.). *Lias.* S. of Fr. (Dufr.).
8. ——— depressa (*Phil.*). *Lias.* Yorks. (Phil.).
9. ——— obliquata (*Sow.*). *Lias.* Mid. and S. Eng. (Conyb.). *Lias.* S. of Fr. (Dufr.). *Lias.* Western Islands, Scotl. (Murch.).
10. ——— cymbium (*Lam.*). *Inf. oolite.* N. of Fr. (Bobl.). *Lias.* S. of France. *Inf. oolite.* Villefranche, S. of France (Dufr.).
11. ——— lituola (*Lam.*). *Brad. clay, cornb., and for. marb.* N. of Fr. (Bobl.).
12. ——— gigantea (*Sow.*). *Lias.* S. of Fr. (Dufr.). *Lias.* Ross and Cromarty, Scotl. *Great arenaceous formation.* Western Islands, Scotl. (Murch.).
13. ——— minuta (*Sow.*). *Great oolite.* Ancliff, Somerset (Cookson).
14. ——— virgula (*Defrance*). *Kim. clay.* Havre (Al. Brong.). *Kim. clay.* Burgundy (Beaum.). *Kim. clay.* S. of Fr. (Dufr.). *Kim. clay.* Weymouth (Buckl. & De la B.).

1. Lingula Beanii (*Phil.*). *Inf. oolite.* Yorks. (Phil.).

1. Spirifer Walcotii (*Sow.*). *Lias.* Yorks. (Phil.). *Lias.* Bath, Lyme Regis (De la B.). *Lias.* Norm. (De C.). *Lias.* S. of Fr. (Dufr.). *Lias.* Western Islands, Scotl. (Murch.).

1. Terebratula intermedia (*Sow.*). *Coral. oolite and great oolite.* Yorks. (Phil.). *Cornb.* Mid. and S. Eng. *Inf. oolite.* Dundry (Conyb.).
2. ——— globata (*Sow.*). *Coral. oolite? Great oolite.* Yorks. (Phil.). *For. marb.* Norm. (De C). *Oolite.* Env. of Bath (Sow.).
3. ——— ornithocephala (*Sow.*). *Coral. oolite and Kell. rock.* Yorks. (Phil.). *Kell. rock, cornb., Lias?* Mid. and S. Eng. *Inf. oolite.* Dundry (Conyb.). *Oxford clay, and lias.* Norm. (De C.). *Inf. oolite.* Uzer, S. of Fr. (Dufr.).
4. ——— ovata (*Sow.*). *Coral. oolite?* Yorks. (Phil.). *Inf. oolite.* Mid. and S. Eng. (Conyb.).
5. ——— obsoleta (*Sow.*). *Coral. oolite? Inf. oolite.* Yorks. (Phil.). *Cornb., Brad. clay, great oolite, and inf. oolite.* Mid. and S. Eng. (Conyb.). *Great oolite.* Norm. (De C.). *Lias and inf. oolite.* S. of Fr. (Dufr.).
6. ——— socialis (*Phil.*). *Calc. grit and Kell. rock.* Yorks. (Phil.).
7. ——— ovoides (*Sow.*). *Cornbrash?* Yorks. (Phil.). *Inf. oolite.* Norm. (De C.). *Rubbly limestone, &c.* Braambury Hill, Brora (Murch.).
8. ——— digona (*Sow.*). *Cornb.* Yorks. (Phil.). *Cornb. and Brad. clay.* Mid. and S. Eng. *Inf. oolite.* Dundry (Conyb.).

For.

For. marb. Norm. (De C.). *Brad. clay and coral rag?* N. of Fr. (Bobl.).

9. Terebratula spinosa (*Townsend and Smith*). *Great oolite.* Yorks. (Phil.).

10. ——— trilineata (*Y. & B.*). *Inf. oolite, and lias.* Yorks. (Phil.).

11. ——— bidens (*Phil.*). *Inf. oolite, and lias.* Yorks. (Phil.).

12. ——— punctata (*Sow.*). *Lias.* Yorks. (Phil.) *Inf. oolite.* Mid. and S. Eng. (Conyb.). *Lias.* Western Islands, Scotl. (Murch.).

13. ——— resupinata (*Sow.*). *Lias.* Yorks. (Phil.). *Inf. oolite.* Mid. and S. Eng. (Conyb.).

14. ——— acuta (*Sow.*). *Lias.* Yorks. (Phil.). *Inf. oolite, and lias.* Mid. and S. Eng. (Conyb.). *Lias.* Norm. (De C.).

15. ——— triplicata (*Phil.*). *Lias.* Yorks. (Phil.)

16. ——— tetraëdra (*Sow.*). *Lias.* Yorks. (Phil.). *Inf. oolite.* Mid. and S. Eng. (Conyb.). *Lias.* S. of Fr. (Dufr.). *For. marb.?* Mauriac, S. of Fr. (Dufr.). *Lias and micaceous sandst.* Western Islands, Scotl. (Murch.).

17. ——— subrotunda (*Sow.*). *Cornb. and inf. oolite.* Mid. and S. Eng. (Conyb.). *Cornb. and for. marb.* N. of Fr. (Bobl.). *For. marb.?* Mauriac, S. of Fr. (Dufr.).

18. ——— obovata (*Sow.*). *Cornb.* Mid. and S. Eng. (Conyb.).

19. ——— reticulata (*Smith*). *Brad. clay.* Mid. and S. Eng. (Conyb.). *For. marb.* Norm. (De C.).

20. ——— carnea (*Sow.*). *Inf. oolite.* Dundry (Conyb.).

21. ——— semigloba (*Sow.*). *Inf. oolite.* Dundry (Conyb.).

22. ——— media (*Sow.*). *Inf. oolite.* Dundry (Conyb.). *Inf. oolite, great oolite, and Brad. clay.* N. of Fr. (Bobl.). *Dunrobin oolite.* Scotl. (Murch.).

23. ——— crumena (*Sow.*). *Inf. oolite and lias?* Mid. and S. Eng. (Conyb.).

24. ——— lateralis (*Sow.*). *Fuller's earth.* Mid. and S. Eng. (Conyb.).

25. ——— concinna (*Sow.*). *Full. E.* Mid. and S. Eng. (Conyb.) *Inf. oolite.* Norm. (De C.). *For. marb.?* Mauriac, S. of Fr. (Dufr.).

26. ——— biplicata (*Sow.*). *Oxford clay, for. marb., great oolite, and inf. oolite.* Norm. (De C.).

27. ——— tetrandra *For. marb.* Norm. (De C.).

28. ——— coarctata (*Sow.*). *For. marb.* Norm. (De C.). *Brad. clay.* N. of Fr. (Bobl.). *Brad. clay.* Bath. (Loscombe.).

29. ——— plicatella (*Sow.*). *For. marb.* Norm. (De C.).

30. ——— serrata (*Sow.*). *For. marb.* Norm. (De C.). *Lias.* Lyme Regis (De la B.).

31. ——— truncata (*Sow.*). *For. marb.* Norm. (De C.).

32. ——— lata (*Sow.*). *Inf. oolite.* Norm. (De C.).

33. ——— dimidiata (*Sow.*). *Inf. oolite.* Norm. (De C.).

34. ——— bullata (*Sow.*). *Inf. oolite.* Norm. (De C.). *Inf. oolite.* Bridport, S. Eng. (*Sow.*).

35. ——— sphæroïdalis (*Sow.*). *Inf. oolite.* Norm. (De C.). *Inf. oolite.* Dundry (Braikenridge).

36. Terebratula

36. Terebratula emarginata (*Sow.*). *Inf. oolite.* Norm. (De C.). *Inf. oolite.* Env. of Bath (Sow.).
37. ——— quadrifida *Lias.* Norm. (De C.).
38. ——— numismalis *Lias.* Norm. (De C.).
39. ——— perovalis (*Sow.*). *Inf. oolite.* Dundry (Braikenridge). *For. marb.?* Mauriac, and *Kim. clay,* Cahors, S. of Fr. *Rochelle limestone* (Dufr.).
40. ——— maxillata (*Sow.*). *Inf. oolite.* Env. of Bath. (Sow.).
41. ——— flabellula (*Sow.*). *Great oolite.* Ancliff, Somerset (Cookson).
42. ——— furcata (*Sow.*). *Great oolite.* Ancliff (Cookson).
43. ——— orbicularis (*Sow.*). *Lias.* Bath (Sow.).
44. ——— hemisphærica (*Sow.*). *Great oolite.* Ancliff (Cookson).
45. ——— inconstans (*Sow.*). *Shell limest. and calc. grit.* Portgower, &c. N. of Scotl., and *shell limestone,* Beal, Isle of Skye (Murch.).

· Cyclas. . . . . . . *Portland stone* (Smith).
Lithodomus *Inf. oolite.* N. of Fr. (Bobl.).

1. Donacites Alduini (*Al. Brong.*). *Inf. oolite?* N. of Fr. (Bobl.). *Kim. clay.* Havre and the Jura (Al. Brong.).

1. Orbicula reflexa (*Sow.*). *Lias.* Yorks. (Phil.).
2. ———? radiata (*Phil.*). *Coral. oolite.* Yorks. (Phil.).
3. ——— granulata (*Sow.*). *Great oolite.* Ancliff, Somerset (Cookson).
——— species not stated. *Inferior oolite.* Yorks. (Phil.).

Delphinula *Coral. oolite and great oolite.* Yorks. (Phil.).

1. Natica aguta (*Smith*). *Coral. oolite.* Yorks. (Phil.).
2. ——— nodulata (*Y. & B.*). *Coral. oolite.* Yorks. (Phil.).
3. ——— cincta (*Phil.*). *Coral. oolite.* Yorks. (Phil.).
4. ——— adducta (*Phil.*). *Great oolite and inf. oolite.* Yorks. (Phil.).
5. ——— tumidula (*Bean*). *Inf. oolite.* Yorks. (Phil.).
——— Species not stated. *Lias.* Yorks. (Phil.).

1. Turbo muricatus (*Sow.*). *Coral. oolite, great oolite, and inf. oolite.* Yorks. (Phil.). *Coral rag.* Mid. and S. Eng. (Conyb.).
2. ——— funiculatus (*Phil.*). *Coral. oolite.* Yorks. (Phil.).
3. ——— sulcostomus (*Phil.*). *Kell. rock.* Yorks. (Phil.).
4. ——— unicarinatus (*Bean*). *Inf. oolite.* Yorks. (Phil.).
5. ——— lævigatus (*Phil.*). *Inf. oolite.* Yorks. (Phil.).
6. ——— undulatus (*Phil.*). *Lias.* Yorks. (Phil.).
7. ——— ornatus (*Sow.*). *Inf. oolite.* Mid. and S. Eng. (Conyb.). *Inf. oolite.* Norm. (De C.).
8. ——— rotundatus (*Sow.*). *Inf. oolite.* Norm. (De C.).
9. ——— obtusus (*Sow.*). *Great oolite.* Ancliff, Somerset (Cookson).
——— species not stated. *Cornb. and great oolite.* Norm. (De C.).

1. Trochus granulatus (*Sow.*). *Coral. oolite, calc. grit, cornb., and inf. oolite.* Yorks. (Phil.). *Inf. oolite.* Dundry (Conyb.). *Inf. oolite.* Norm. (De C.).
2. ———? tornatus (*Phil.*). *Coral. oolite.* Yorks. (Phil.).
3. ——— bicarinatus (*Sow.*). *Calc. grit.* Yorks. (Phil.). *Coral rag.* Mid. and S. Eng. *Inf. oolite.* Dundry (Conyb.). *Inf. oolite.* Norm. (De C.).
4. ——— guttatus (*Phil.*). *Kell. rock.* Yorks. (Phil.).

5. Trochus

5. Trochus monilitectus (*Phil.*). *Great oolite.* Yorks. (Phil.).
6. ——— bisertus (*Phil.*). *Inf. oolite.* Yorks. (Phil.).
7. ——— pyramidatus (*Bean*). *Inf. oolite.* Yorks. (Phil.).
8. ——— anglicus (*Sow.*). *Lias.* Yorks. (Phil.). *Lias.* Mid. and S. Eng. (Conyb.).
9. ——— similis (*Sow.*). *Inf. oolite.* Dundry. And *lias.* Mid. and S. Eng. (Conyb.).
10. ——— concavus (*Sow.*). *Inf. oolite.* Mid. and S. Eng. (Conyb.). *Inf. oolite.* Norm. (De C.).
11. ——— dimidiatus (*Sow.*). *Inf. oolite.* Mid. and S. Eng. (Conyb.).
12. ——— duplicatus (*Sow.*). *Inf. oolite.* Mid. and S. Eng. (Conyb.).
13. ——— elongatus (*Sow.*). *Inf. oolite.* Dundry (Conyb.). *For. marb. and inf. oolite.* Norm. (De C.).
14. ——— punctatus (*Sow.*). *Inf. oolite.* Dundry (Conyb.). *Inf. oolite.* Norm. (De C.).
15. ——— abbreviatus (*Sow.*). *Inf. oolite.* Dundry (Conyb.). *Inf. oolite.* Norm. (De C.).
16. ——— fasciatus (*Sow.*). *Inf. oolite.* Dundry (Conyb.). *Inf. oolite.* Norm. (De C.).
17. ——— sulcatus (*Sow.*). *Inf. oolite.* Dundry (Conyb.). *Inf. oolite.* Norm. (De C.).
18. ——— ornatus (*Sow.*). *Inf. oolite.* Dundry (Conyb.). *Inf. oolite.* Norm. (De C.) *Lias.* N. of Fr. (Bobl.).
19. ——— imbricatus (*Sow.*). *Lias.* Mid. and S. Eng. (Conyb.). *Inf. oolite.* Norm. (De C.). *Lias.* S. of Fr. (Dufr.).
20. ——— Gibsii (*Sow.*). *Oxford clay.* Norm. (De C.).
21. ——— reticulatus (*Sow.*). *Inf. oolite.* Norm. (De C.).
——— species not stated. *Portland stone and Bradford clay.* Mid. and S. Eng. (Conyb.). *Coral rag.* Norm. (De C.).

1. Turritella muricata (*Sow.*). *Coral. oolite, calc. grit, Kell. rock, and inf. oolite.* Yorks. (Phil.). *Rochelle limestone* (Dufr.). *Shell limestone and grit.* Portgower, &c. Scotland (Murch.).
2. ——— cingenda (*Sow.*). *Coral. oolite? great oolite, and inf. oolite.* Yorks. (Phil.).
3. ——— quadrivittata (*Phil.*). *Inf. oolite.* Yorks. (Phil.).
4. ——— concava (*Sow.*). *Portland stone.* Tisbury (Benett).
——— species not stated. *Portland stone, coral rag? cornb., for. marb., Brad. clay.* Mid. and S. Eng. (Conyb.). *Brad. clay.* N. of Fr. (Bobl.).

1. Myoconcha crassa (*Sow.*). *Inf. oolite.* Norm. (De C.). *Inf. oolite.* Dundry (Sow.).

1. Terebra melanoïdes (*Phil.*). *Coral. oolite.* Yorks. (Phil.).
2. ——— ? granulata (*Phil.*). *Coral. oolite and cornb.* Yorks. (Phil.).
3. ——— vetusta (*Phil.*). *Great oolite and inf. oolite.* Yorks. (Phil.).
4. ——— sulcata *Coral rag.* N. of Fr. (Bobl.).

1. Rissoa lævis (*Sow.*). *Great oolite.* Ancliff, Somerset (Cookson).
2. ——— acuta (*Sow.*). *Great oolite.* Ancliff (Cookson).
3. ——— obliquata (*Sow.*). *Great oolite.* Ancliff (Cookson).
4. ——— duplicata (*Sow.*). *Great oolite.* Ancliff (Cookson).

Ancilla, species not stated. *Great oolite and for. marb.* Norm. (De C.).

1. Emarginula scalaris (*Sow.*). *Great oolite.* Ancliff, Somerset (Cookson).

2. Melania

1. Melania Heddingtonensis (*Sow.*). *Coral. oolite, cornb., great oolite, and inf. oolite.* Yorks. (Phil.). *Coral rag.* Midl. and S. Engl. *Inf. oolite.* Dundry (Conyb.). *Coral rag and inf. oolite.* Norm. (De C.). *Rubbly limestone, &c.* Braambury Hill, Brora (Murch.). *Kim. clay.* Havre (Phil.).
2. ——— striata (*Sow.*). *Coral. oolite and great oolite?* Yorks. (Phil.). *Coral rag and lias.* Midl. and S. Engl. (Conyb.). *Coral rag.* N. of Fr. (Bobl.). *Kim. clay.* Havre (Phil.).
3. ——— vittata (*Phil.*). *Cornb.* Yorks. (Phil.).
4. ——— lineata (*Sow.*). *Inf. oolite.* Yorks. (Phil.). *Inf. oolite.* Dundry (Conyb.). *Inf. oolite.* Norm. (De C.).

——— species unknown. *Great oolite.* Midl. and S. Engl. (Conyb.).

1. Bulla elongata (*Phil.*). *Coral. oolite.* Yorks. (Phil.).
1. Murex Haccanensis (*Phil.*). *Coral. oolite.* Yorks. (Phil.).
1. Cirrus cingulatus (*Phil.*). *Calc. grit.* Yorks. (Phil.).
2. ——— depressus (*Phil.*). *Kell. rock.* Yorks. (Phil.).
3. ——— nodosus (*Sow.*). *Inf. oolite.* Dundry (Conyb.).
4. ——— Leachii (*Sow.*). *Inf. oolite.* Dundry (Conyb.).

——— species undetermined. *Lias.* N. of Fr. (Bobl.).

1. Actæon retusus (*Phil.*). *Calc. grit.* Yorks. (Phil.).
2. ——— glaber (*Bean*). *Great oolite and inf. oolite.* Yorks. (Phil.).
3. ——— humeralis (*Phil.*). *Inf. oolite.* Yorks. (Phil.).
4. ——— cuspidatus (*Sow.*). *Great oolite.* Ancliff, near Bath (Cookson).
5. ——— acutus (*Sow.*). *Great oolite.* Ancliff, near Bath (Cookson).

——— species not stated. *Lias.* Yorks. (Phil.).

Nerinea, species not stated. *Coral rag and for. marb.* Norm. (De C.). *Brad. clay.* N. of Fr. (Bobl.).

1. Rostellaria bispinosa *Calc. grit? and Kell. rock.* Yorks. (Phil.).
2. ——— trifida (*Bean*). *Oxford clay.* Yorks. (Phil.).
3. ——— Parkinsonii (*Sow.*). *Inf. oolite.* Norm. (De C.).
4. ——— composita (*Sow.*). *Sandst., limest., and shale.* Inverbrora, Scotl. (Murch.). *Great? inf. oolite.* Yorks. (Phil.). *Oxford clay.* Weymouth (Sow.). *Kim. clay.* Havre (Phil.).

——— species not stated. *Lias.* Yorks. (Phil.). *Oxford clay, Kell. rock, cornb., forest marb., and inf. oolite.* Mid. and S. Eng. (Conyb.). *Oxford clay.* Norm. (De C.).

1. Pteroceras oceani (*Al. Brong.*). *Kim. clay.* Havre, the Jura, and the Perte du Rhone (Al. Brong.). *Great oolite.* Alsace (Voltz).
2. ——— ponti (*Al. Brong.*). *Kim. clay.* Havre and the Jura (Al. Brong.).
3. ——— Pelagi (*Al. Brong.*). *Kim. clay.* Havre and the Jura (Al. Brong.).

1. Patella latissima (*Sow.*). *Oxford clay.* Yorks. (Phil.). *Oxford clay.* Mid. and S. Eng. (Conyb.).
2. ——— rugosa (*Sow.*). *Forest marble.* Mid. and S. Eng. (Conyb.). *For. marb.* Norm. (De C.).
3. ——— lævis (*Sow.*) *Lias.* Mid. and S. Eng. (Conyb.).
4. ——— lata (*Sow.*). *Stonesfield slate* (Sow.).
5. ——— ancyloïdes (*Sow.*). *Great oolite.* Ancliff, near Bath (Cookson)
6. ——— nana (*Sow.*). *Great oolite.* Ancliff, near Bath (Cookson).

1. Phasianella cincta (*Phil.*). *Great oolite.* Yorks. (Phil.).
1. Solarium calix (*Bean*). *Inf. oolite.* Yorks. (Phil.).
2. ——— conoideum (*Sow.*). *Portland stone* (Conyb.).
1. Nerita costata (*Sow.*). *Inf. oolite.* Yorks. (Phil.). *Great oolite.* Ancliff, near Bath (Cookson).
2. ——— sinuosa (*Sow.*). *Portland stone* (Conyb.).
3. ——— lævigata (*Sow.*). *Inf. oolite.* Dundry (Conyb.). *Shell limestone and calc. grit.* Portgower, &c., Scotland (Murch.).

4. Nerita

4. Nerita minuta (*Sow.*). *Great oolite.* Ancliff, near Bath (Cookson).
1. Auricula Sedgevici (*Phil.*). *Inf. oolite.* Yorks. (Phil.).
1. Buccinum unilineatum (*Sow.*). *Great oolite.* Ancliff, near Bath (Cookson).
——— species unknown. *Shale, sandst., and limest.* Inverbrora, Scotl. (Murch.).

Tornatilla, species unknown. *Lias.* Mid. and S. Eng. (Conyb.).

Ampullaria, species unknown. *Coral rag, cornb., and inf. oolite.* Mid. and S. Eng. (Conyb.). *Coral rag.* Norm. (De C.). *Brad. clay.* N. of Fr. (Bobl.).

1. Planorbis euomphalus (*Sow.*). *Inf. oolite.* Mid. and S. Eng. (Conyb.).
1. Helicina polita (*Sow.*). *Inf. oolite.* Cropredy (Conyb.).
2. ——— compressa (*Sow.*). *Lias.* Mid. and S. Eng. (Conyb.).
3. ——— expansa (*Sow.*). *Lias.* Mid. and S. Eng. (Conyb.).
4. ——— solarioides (*Sow.*). *Lias.* Mid. and S. Eng. (Conyb.).
1. Belemnites sulcatus (*Mill.*) *Coral oolite? calc. grit, Oxford clay, and Kell. rock.* Yorks. (Phil.). *Shale, sandst., and limest.* Inverbrora, Scotl. (Murch.).
2. ——— fusiformis (*Mill.*). *Coral. oolite?* Yorks. (Phil.).
3. ——— gracilis (*Phil.*). *Oxford clay.* Yorks. (Phil.).
4. ——— abbreviatus (*Mill.*). *Great oolite.* Yorks. (Phil.). *Lias.* Ross and Cromarty, Scotl., and *Micaceous sandst.* Western Islands, Scotl. (Murch.).
5. ——— elongatus (*Mill.*). *Lias.* Yorks. (Phil.). *Lias.* Ross and Cromarty, Scotl. (Murch.).
6. ——— trisulcatus (*Blainville*). *Inf. oolite.* N. of Fr. (Bobl.).
7. ——— compressus (*Sow.*). *Fuller's E.* N. of Fr. (Bobl.). *Inf. oolite.* Yorks. (Sow.).
8. ——— dilatatus *Fuller's E.* N. of Fr. (Bobl.).
9. ——— apicicurvatus (*Bl.*). *Lias.* S. of Fr. (Dufr.). *Lias.* Alais. (Al. Brong.).
10. ——— sulcatus *Lias.* S. of Fr. (Dufr.). Reefs at Dunrobin, Scotl. (Murch.).
11. ——— pistilliformis (*Blain.*). *Lias.* S. of Fr. (Dufr.).
12. ——— brevis (*Blain.*). *Lias.* Alais (Brong.).

Belemnites, species not stated. *Kim. clay and inf. oolite.* Yorks. (Phil.). *Kim. clay, coral rag, Oxford clay, Kell. rock, Stonesfield slate, Bradford clay, and inf. oolite.* Mid. and S. Eng. (Conyb.). *Oxford clay, for. marb., great oolite, inf. oolite, and lias.* Norm. (De C.). *Lias.* N. of Fr. (Bobl.).

1. Turrilites Babeli *Brong.*). *Coral rag?* N. of Fr. (Bobl.).
1. Orthoceras elongatum (*De la B.*). *Lias.* Lyme Regis (De la B.).
1. Nautilus hexagonus (*Sow.*). *Kell. rock?* Yorks. (Phil.). *Calc. grit.* Oxford (Sow.).
2. ——— lineatus (*Sow.*). *Inf. oolite and lias.* Yorks. (Phil.). *Inf. oolite.* Dundry (Conyb.).
3. ——— astacoides (*Y. & B.*). *Lias.* Yorks. (Phil.).
4. ——— annularis (*Phil.*). *Lias.* Yorks. (Phil.).
5. ——— obesus (*Sow.*). *Inf. oolite.* Midl. and S. Engl. (Conyb.). *Inf. oolite.* Norm. (De C.).
6. ——— sinuatus (*Sow.*). *Inf. oolite.* Midl. and S. Engl. (Conyb.). *Oxford clay.* Norm.? (De la B.).
7. ——— intermedius (*Sow.*). *Lias.* Midl. and S. Engl. (Conyb.).
8. ——— striatus (*Sow.*). *Lias.* Midl. and S. Engl. (Conyb.). *Lias.* Alsace (*Brong.*).
9. ——— truncatus (*Sow.*) *Lias.* Midl. and S. Engl. (Conyb.). *For. marble and lias.* Norm. (De Cau.).

10. Nautilus

10. Nautilus angulosus (*D'Orbigny*). *Portland stone.* Isle d'Aix (Brong.).
——— species not stated. *Great oolite.* Yorks. (Phil.). *Kim. clay, coral rag, Oxford clay, Kell. rock, Stonesfield slate.* Midl. and S. Engl. (Conyb.). *Coral rag.* Norm. (De Cau.). *Fuller's earth.* N. of Fr. (Bobl.).

1. Ammonites perarmatus (*Sow.*). *Coral. oolite, calc. grit, and Kell. rock.* Yorks. (Phil.). *Oolitic rocks.* Braambury Hill, Brora (Murch.).
2. ——— plicomphalus (*Sow.*). *Kim. clay?* Yorks. (Phil.). *Oxford clay.* Norm. (De C.).
3. ——— triplicatus (*Sow.*). *Coral. oolite.* Yorks. (Phil.). *Portland-stone* (Conyb.). *Inf. oolite.* Norm. (De C.).
4. ——— plicatilis (*Sow.*). *Coral. oolite, and Kell. rock.* Yorks. (Phil.). *Coral. rag.* Midl. and S. Engl. (Conyb.).
5. ——— Williamsoni (*Phil.*). *Coral. oolite.* Yorks. (Phil.).
6. ——— Sutherlandiæ (*Sow.*). *Sandstone.* Braambury Hill, Brora (Murch.). *Coral. oolite, and calc. grit.* Yorks. (Phil.).
7. ——— sublævis (*Sow.*). *Coral. oolite, Kell. rock.* Yorks. (Phil.). *Kell. rock.* Midl. and S. Engl. (Conyb.). *Oxford clay.* Norm. (De la B.).
8. ——— lenticularis (*Phil.*). *Coral. oolite? Kell. rock, and lias* Yorks. (Phil.).
9. ——— vertebralis & cordatus (*Sow.*). *Coral. oolite, calc. grit, and Oxford clay.* Yorks. (Phil.). *Coral rag.* Midl. and S. Engl. (Conyb.). *Oolite* of Braambury Hill, Brora (Murch.).
10. ——— instabilis (*Phil.*). *Calc. grit.* Yorks. (Phil.).
11. ——— solaris (*Phil.*). *Calc. grit.* Yorks. (Phil.).
12. ——— oculatus (*Phil.*). *Oxford clay.* Yorks. (Phil.).
13. ——— Vernoni (*Bean*). *Oxford clay.* Yorks. (Phil.).
14. ——— athleta (*Phil.*). *Oxford clay and Kell. rock.* Yorks. (Phil.).
15. ——— Kœnigi (*Sow.*). *Kell. rock.* Yorks. (Phil.). *Kell. rock.* Midl. and S. Engl. (Conyb.). *Micaceous sandst.* Western Islands, Scot. (Murch.).
16. ——— bifrons (*Phil.*). *Kell. rock.* Yorks. (Phil.).
17. ——— Gowerianus (*Sow.*). *Shale, sandst. and limest.* Inverbrora, Scotl. (Murch.). *Kell. rock.* Yorks. (Phil.).
18. ——— Calloviensis (*Sow.*). *Kell. rock.* Yorks. (Phil.). *Kell. rock.* Mid. and S. Engl. (Conyb.).
19. ——— Duncani (*Sow.*). *Kell. rock.* Yorks. (Phil.). *Oxford clay.* Midl. and S. Engl. (Conyb.). *Oxford clay.* Norm. (De Cau.).
20. ——— gemmatus (*Phil.*). *Kell. rock.* Yorks. (Phil.).
21. ——— Herveyi (*Sow.*). *Kell. rock? cornb.* Yorks. (Phil.). *Inf. oolite.* Midl. and S. Engl. (Conyb.)
22. ——— flexicostatus (*Phil.*). *Kell. rock.* Yorks. (Phil.).
23. ——— funiferus (*Phil.*). *Kell. rock.* Yorks. (Phil.).
24. ——— terebratus (*Phil.*). *Cornbrash.* Yorks. (Phil.).
25. ——— Blagdeni (*Sow.*). *Great oolite.* Yorks. (Phil.). *Inf. oolite.* Dundry (Conyb.). *Inf. oolite.* Norm. (De Cau.).
26. ——— striatulus (*Sow.*). *Inf. oolite and lias.* Yorks. (Phil.).
27. ——— heterophyllus (*Sow.*). *Lias.* Yorks. (Phil.). *Lias.* Midl. and S. Engl. (Conyb.).
28. ——— subcarinatus (*Y. & B.*). *Lias.* Yorks. (Phil.).
29. ——— Henleyi (*Sow.*). *Lias.* Yorks. (Phil.). *Lias.* Midl. and S. Engl. (Conyb.).
30. ——— heterogeneus (*Y. & B.*). *Lias.* Yorks. (Phil.).

31. Ammonites crassus (*Y. & B.*). *Lias.* Yorks. (Phil.).
32. ———— communis (*Sow.*). *Lias.* Yorks. (Phil.). *Lias.* Midl. and S. Engl. (Conyb.). *Lias.* Western Islands, Scotl. (Murch.).
33. ———— angulatus (*Sow.*). *Lias.* Yorks. (Phil.). *Lias.* Midl. and S. Engl. (Conyb.).
34. ———— annulatus (*Sow.*). *Lias.* Yorks. (Phil.). *Inf. oolite, and lias* Midl. and S. Engl. (Conyb.). *Oxford clay, forest marble, inf. oolite.* Norm. (De C.). *Inf. oolite.* Uzer, S. of France. *Rochelle limestone.* (Dufr.). *Inf. oolite and lias.* Montdor, Lyon (Al. Brong.).
35. ———— fibulatus (*Sow.*). *Lias.* Yorks. (Phil.).
36. ———— subarmatus (*Sow.*). *Lias.* Yorks. (Phil.).
37. ———— maculatus (*Y. & B.*). *Lias.* Yorks. (Phil.).
38. ———— gagateus (*Y. & B.*). *Lias.* Yorks. (Phil.).
39. ———— planicostatus (*Sow.*). *Lias.* Yorks. (Phil.). *Lias.* Midl. and S. Engl. (Conyb.).
40. ———— balteatus (*Phil.*). *Lias.* Yorks. (Phil.).
41. ———— arcigerens (*Phil.*). *Lias.* Yorks. (Phil.).
42. ———— brevispina (*Sow.*). *Lias.* Western Islands, Scotl. (Murch.). *Lias.* Yorks. (Phil.).
43. ———— Jamesoni (*Sow.*). *Lias.* Western Islands, Scot. (Murch.). *Lias.* Yorks. (Phil.).
44. ———— erugatus (*Bean*). *Lias.* Yorks. (Phil.). *La Spezia limestones* (De la B.).
45. ———— fimbriatus (*Sow.*). *Lias.* Yorks. (Phil.). *Lias.* Midl. and S. Eng. (Conyb.). *Lias.* Norm. (De C.).
46. ———— nitidus (*Y. & B.*). *Lias.* Yorks. (Phil.).
47. ———— anguliferus (*Phil.*). *Lias.* Yorks. (Phil.).
48. ———— crenularis (*Phil.*). *Lias.* Yorks. (Phil.).
49. ———— Clevelandicus (*Y. & B.*). *Lias.* Yorks. (Phil.).
50. ———— Turneri (*Sow.*). *Lias.* Yorks. (Phil.). *Lias.* S. of Fr. (Dufr.)
51. ———— geometricus (*Phil.*). *Lias.* Yorks (Phil.).
52. ———— vittatus (*Y. & B.*) *Lias.* Yorks. (Phil.).
53. ———— sigmifer (*Phil.*). *Lias.* Yorks. (Phil.).
54. ———— Hawskerensis (*Y. & B.*). *Lias.* Yorks. (Phil.).
55. ———— Conybeari (*Sow.*). *Lias.* Yorks. (Phil.). *Lias.* Midl. and S. Engl. (Conyb.). *Lias.* Alsace: Gundershofen and Buxweiller (Al. Brong.). *Lias.* Western Islands, Scotl. (Murch.).
56. ———— Bucklandi (*Sow.*). *Lias.* Yorks. (Phil.). *Lias.* Midl. and S. Engl. (Conyb). *Lias.* Norm. (De Cau.).
57. ———— obtusus (*Sow.*). *Lias.* Yorks. (Phil.). *Lias.* Midl. and S. Engl. (Conyb.).
58. ———— Walcotii (*Sow.*). *Lias.* Yorks. (Phil.). *Inf. oolite and lias.* Midl. and S. Engl. (Conyb.). *Lias.* Norm. (De C.). *Lias.* S. of Fr. (Dufr.).
59. ———— ovatus (*Y. & B.*). *Lias.* Yorks. (Phil.).
60. ———— Mulgravius (*Y. & B.*). *Lias.* Yorks. (Phil.).
61. ———— exaratus (*Y. & B.*). *Lias.* Yorks. (Phil.).
62. ———— Lythensis (*Y. & B.*). *Lias.* Yorks. (Phil.).
63. ———— concavus (*Sow.*). *Lias?* Yorks. (Phil.). *Inf. oolite.* Mid. and S. Engl. (Conyb.). *Lias.* Norm. (De C.).
64. ———— elegans (*Sow.*). *Lias?* Yorks. (Phil.). *Inf. oolite.* Dundry (Conyb.). *Lias.* Norm. (De C.). *Inf. oolite.* Uzer, S. of Fr. (Dufr.).

65. Ammonites discus (*Sow.*). *Inf. oolite.* Dundry. *Cornb.* Mid. and S. Engl. (Conyb.). *Inf. oolite.* Norm. (De C.).
66. ———— Banksii (*Sow.*). *Inf. oolite.* Dundry (Conyb.).
67. ———— Braikenridgii (*Sow.*). *Inf. oolite.* Dundry (Conyb.). *Inf. oolite.* Norm. (De C.).
68. ———— Brocchii (*Sow.*). *Inf. oolite.* Dundry (Conyb.).
69. ———— Sowerbii (*Miller.*). *Inf. oolite.* Dundry (Conyb.).
70. ———— falcifer (*Sow.*). *Inf. oolite.* Dundry (Conyb.). *Lias.* Norm. (De C.). *Lias.* S. of Fr. (Dufr.).
71. ———— Brownii (*Sow.*). *Inferior oolite.* Dundry (Conyb.).
72. ———— læviusculus (*Sow.*). *Inferior oolite.* Dundry (Braikenridge). *Inf. oolite.* Norm. (De C.).
73. ———— acutus (*Sow.*). *Oxford clay, inf. oolite.* Norm. (De C.). *Lias.* Western Islands, Scotl. (Murch.).
74. ———— contractus (*Sow.*). *Inf. oolite.* Dundry (Sow.). *Inf. oolite.* Norm. (De C.).
75. ———— giganteus (*Sow.*). *Portland stone, coral rag and lias.* Mid. and Engl. (Conyb.). *Portland stone.* Isle d'Aix (Brong.).
76. ———— Lamberti (*Sow.*). *Portland stone* (Conyb.). *Rochelle limestone* (Dufr.).
77. ———— Nutfieldiensis (*Sow.*). *Portland stone* (Conyb.).
78. ———— excavatus (*Sow.*). *Coral rag.* Mid. and S. Engl. (Conyb.). *Oxford clay.* Norm. (De la B.). *Lias.* Norm. (De C.).
79. ———— splendens (*Sow.*). *Coral rag.* Mid. and S. Engl. (Conyb.).
80. ———— armatus (*Sow.*). *Oxford clay, and lias.* Mid. and S. Engl. (Conyb.). *Oxford clay.* Norm. (De la B.).
81. ———— modiolaris (*Sow.*). *Fuller's earth?* Mid. and S. Engl. (Conyb.).
82. ———— jugosus (*Sow.*). *Inf. oolite.* Mid. and S. Eng. (Conyb.).
83. ———— Stokesii (*Sow.*). *Inf. oolite.* Mid. and S. Engl. (Conyb.). *Lias.* Norm. (De C.). *Lias.* S. of Fr. (Dufr.).
84. ———— Strangewaysii (*Sow.*). *Inf. oolite.* Mid. and S. Engl. (Conyb.). *Lias.* Norm. (De C.).
85. ———— falcatus (*Sow.*). *Inf. oolite.* Mid. and S. Engl. (Conyb.).
86. ———— Brookii (*Sow*). *Inf. oolite & lias.* Mid. and S. Engl. (Con.).
87. ———— Bechii (*Sow.*). *Inferior oolite and lias.* Mid. and S. Eng. (Conyb.). *Lias.* Lyme Regis (De la B.).
88. ———— stellaris (*Sow.*). *Lias.* Mid. and S. Engl. (Conyb.). *Lias.* Norm. (De C.).
89. ———— Greenovii (*Sow.*). *Lias.* Mid. and S. Engl. (Conyb.). *Lias.* Lyme Regis (De la B.).
90. ———— Loscombi (*Sow.*). *Lias.* Mid. and S. Engl. (Conyb.). *Lias.* Lyme Regis (De la B.).
91. ———— Birchii (*Sow.*). *Lias.* Mid. and S. Engl. (Conyb.). *Lias.* Lyme Regis (De la B.).
92. ———— omphaloides (*Sow.*). *Oxford clay.* Norm. (De la B.). *Gt. arenaceous formation.* Western Islands, Scotl. (Murch.).
93. ———— quadratus. *Inf. oolite.* Norm. (De C.).
94. ———— Gervillii (*Sow.*). *Inf. oolite.* Norm. (De C.).
95. ———— Brongniartii (*Sow.*). *Inf. oolite.* Norm. (De C.).
96. ———— biplex (*Sow.*). *Inf. oolite.* Norm. (De C.). *Lias.* Ross and Cromarty, Scotl. (Murch.).
97. ———— rotundus (*Sow.*). *Inf. oolite.* Norm. (De C.). *Kim. clay.* Purbeck (Sow.).
98. ———— complanatus. *Inf. oolite.* Norm. (De C.).
99. ———— decipiens. *Lias.* Norm. (De C.).
100. ———— Deslongchampi. *Inf. oolite.* N. of Fr. (Bobl.).

 101. Ammonites

101. Ammonites vulgaris. *Bradford clay.* N. of Fr. (Bobl.).
102. ——— coronatus. *Oxford clay?* N. of Fr. (Bobl.).
103. ——— Humphresianus (*Sow*). *Lias.* S. of Fr. (Dufr.). *Inf. oolite.* Sherborne (Sow.).
104. ——— Parkinsoni (*Sow.*). *Lias.* Bath (Sow.).
105. ——— Gulielmii (*Sow.*). *Oxford clay.* S. Engl. (Sow.).
106. ——— Davæi (*Sow.*). *Lias.* Lyme Regis (De la B.).
107. ——— planorbis (*Sow.*). *Lias.* Watchet, Somerset (Sow.).
108. ——— Johnstonii (*Sow.*). *Lias.* Watchet, Somerset (Sow.).
109. ——— corrugatus (*Sow.*). *Inf. oolite.* Dundry (Braikenridge).
110. ——— rotiformis (*Sow.*). *Lias.* Yeovil (*Sow.*).
111. ——— multicostatus (*Sow.*). *Lias.* Bath (Sow.).
112. ——— lævigatus (*Sow.*). *Lias.* Lyme Regis (De la B.).
113. ——— latæcosta (*Sow.*). *Lias.* Lyme Regis (Murch.).
114. ——— Murchisonæ (*Sow.*). *Micaceous sandst.* Holm Cliff, Western Islands, Scotl. (Murch.). *Inf. oolite.* Allington, near Bridport (Murch.).

Ammonites, species not mentioned. *Coral rag, great oolite.* Norm. (De C.). *Lias, inf. oolite, Fuller's earth.* N. of Fr. (Bobl.). *Kim. clay.* Yorks. (Phil.).

[To be continued.]

VIII. *Notices occasioned by the Perusal of a late Publication by Mr. Babbage.* *By* Capt. Edward Sabine, *Roy. Art. Sec. R.S.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

YOUR insertion in the Philosophical Magazine of the following notices, occasioned by the perusal of Mr. Babbage's book, will oblige me. I regret that this communication was not in time for your Magazine of last month; but not having been able to obtain an extension of my leave of absence, which expired on the 30th of May, and being under the necessity of leaving London in order to join my regiment, it was not in my power to prepare it sooner. I am, Gentlemen,

Your obedient servant,

Charlemont, Ireland, June 6, 1830. Edward Sabine.

1. Being at Paris in the spring of 1827, I received a letter from Captain Kater, acquainting me that he had ascertained the value of the divisions of the level of a small repeating circle, which I had used to observe zenith distances for latitude and time at some of my pendulum stations, to be 10·9 seconds each, instead of single seconds, which Mr. Dollond the maker had mentioned as their value, when I received the instrument from him. I lost no time in recalculating all the observations made with the circle, and, returning to England soon afterwards, gave a paper to the Royal Society, and inserted a letter in the Philosophical Magazine and Annals *; in

* See Phil. Mag. & Annals, N. S. vol. ii. pp. 124, 143, & 176. Edit.

in both of which I communicated the corrected lengths of the pendulum, and the latitudes: and showed by their comparison with those previously calculated, that the differences were far too small to have any influence whatsoever on any of the deductions in which those results had been or were likely to be employed. That, in fact, for every practical purpose regarding the pendulum experiments in which the circle was used, it was immaterial whether the divisions of the level were considered as single seconds, or as 10·9 seconds each.

Regarding the divisions as single seconds when observing with the circle, I had deemed it of less consequence that the readings of the level should be a precise record, than I should have done had I known their true value. Satisfied if the readings should approximate within limits which, as I believed, would not occasion error of greater amount than the results were subject to from other causes, I made no comment beyond the notice of the fact, being occasionally obliged to employ the circle on supports that were not perfectly stable or insulated; such as on window sills, &c., such positions being on those occasions the best within my command. For the same reason I suffered myself greatly to underrate the real disadvantage I laboured under, in not having, as is usual in observations with a repeating circle, an assistant, whose observation should be exclusively given to the level, and who should read it simultaneously with the observation at the telescope, to which moment the reading should correspond. Having both operations to perform myself, I was obliged to attend to them in succession, and thus to read the level subject to such small alterations as the changes of my own position or other causes might have occasioned in the interim*.

I may here remark, that had I been acquainted with the true value of the level at the time, I should still have employed the circle when I did; for it was the best resource I had, and was amply sufficient for the purposes of the pendulum, for which alone I required its use. But I should

* I had trained my servant, who had been with me in the northern voyages, to render me the assistance alluded to, and other assistance of the same nature: but I had the misfortune to lose his services at Sierra Leone, my first station, by the fever of the country, from the effects of which he ultimately died. Capt. Clavering, in his anxiety to promote the objects in which I was engaged, endeavoured, as far as he was able, to remedy the loss, by supplying me with assistants chosen from amongst the marines of his ship's company; but after witnessing the deaths of five men thus landed at different times for my assistance, I determined from thenceforward to do the best I could alone, rather than obtain assistance at such fearful risk to others.

not have deemed the observations made with it, under circumstances that necessarily rendered the readings of the level an approximate instead of an exact record, a specimen of what the circle was capable of performing when an attention should be given to the level, commensurate with the greater value of its divisions.

The circumstance noticed by Mr. Babbage, that some of the latitudes observed with the circle agree better together when computed with the smaller and erroneous value of the level, than when computed with the larger and more correct value, is a natural consequence of the readings of the level being approximate and not exact.

To exemplify this, let it be supposed that the sum of the readings of the level may have had an accumulated error, in an extreme case, of ten divisions; that is to say, an error of +10 divisions, or of −10 divisions from the truth. Now let there be observations of two stars, in all respects precisely exact, except that the one star be charged with the positive, and the other with the negative, error; let the number of repetitions be 10: then will the resulting latitudes be charged, in the one case with an error of +1 division of the level, and in the other case of −1 division. When the divisions are reckoned as single seconds, the effect of this error will be to remove the latitudes 2 seconds apart; but with the greater multiplier of 10·9, they will be removed nearly 22 seconds apart.

Hence if there are several observations at a station, and the observations are good in all respects except in the liability to error, within the above named limits, in the record of the level, the latitudes will have very small differences when computed with the small but erroneous value of the level; and the differences will be increased tenfold in amount when the larger and more correct value is introduced.

As might be expected the differences in the recalculated latitudes are most remarkable at the stations where the support of the circle was least stable: at New York, where it stood in the window of the cupola of Columbia Cottage; at Bahia, where it was placed upon a garden table; and at Maranham, where, although great care was taken in all respects then deemed of most importance,—the position of the circle on the window sill of the apartment was unfavourable in regard to stability, and probably influenced the two observations of *α* Lyræ, which depart each about 10 seconds from the mean*. At Spitzbergen, on the contrary, where in addition to the same case

* The observations at Maranham, recalculated with 10·9, and employing the

case in other respects as at Maranham, the circle rested on a firm basis of rock, the recalculation shows that the record of the level was unusually free from error. The observations at Spitzbergen recalculated with 10·9 are as follows:

	Southern Meridian.		*Northern Meridian.*
1823. July 5.	Sun... 79° 49′ 58″·6	July 6.	Sun... 79° 49′ 44″·8
12.	—... 79 49 57 ·0	7.	—... 79 49 52 ·7
		9.	—... 79 49 53 ·6
Mean; noon obs.	79 49 57 ·8	10.	—... 79 49 51 ·6
		Mean; midnight observations...	79 49 50 ·7

Mean latitude from northern and southern observations 79° 49′ 54″·25.

The mean latitude with the same observations calculated with the divisions of the level as single seconds is 79° 49′ 57″·8. The recalculation with 10·9 manifests a slight defect in the vertical adjustment of the circle, which defect was previously masked; and the observations being separated into northern and southern results, are found to agree, with a single exception, and that not to a great amount, even better than they appeared to do before.

The observations with the circle were originally noted down in pencil on detached pieces of paper; they were calculated, and transcribed in the form in which they are printed, either at the station itself, or on the passage to the next station; the transcript was made in duplicate, and one of the copies sent home. The original pencil record may not always have been preserved after the transcript was made; and of those then preserved, some have probably been lost in the years that have since passed, as I have never had the advantage of a fixed residence, and have frequently been obliged to move all that I possessed from place to place. I have been so fortunate, however, as to find several of these original papers in searching in the last few days; amongst them are three of the four latitudes

the apparent places of the two southern stars, as furnished to me a few days since by Mr. Taylor, of the Royal Observatory, from the latest observations, are as follows:

1822. August 28, α Lyræ......	2° 31′ 22″		
—— 29, α Lyræ......	2 31 31·8		
—— 29, α Pavonis...	2 31 31·4	Æ 20° 11′ 37″; Decl. 57 17 27	
—— 31, α Lyræ......	2 31 42·6		
—— 31, α Cygni.....	2 31 38·8		
Sept. 2, α Gruis.....	2 31 34·8	Æ 21 57 04; Decl. 47 48 42	
Mean....	2° 31 33·6		

latitudes observed at New York, which are amongst the most remarkable instances of agreement when calculated with single seconds as the value of the level, and of separation when recalculated with the value of 10·9. They are as follows:

	Level, single Seconds.	*Level, 10·9 Seconds.*	
1822. Dec. 24. Sun	40° 42′ 40″·1	40° 42′ 44″·6	Originals existing.
31. Sun	40 42 41 ·4	40 42 47 ·2	
1823. Jan. 3. B. Ursæ	40 42 42 ·3	40 42 58 ·4	
1822. Dec. 24. Polaris.	40 42 48 ·9	40 42 38 ·2	Original not found.
Mean; level single seconds	40 42 43 ·2	M.; level 10·9 secds 40 42 47 ·1	

There are also the originals of four of the six latitudes at Maranham; one of these is the observation of *α* Lyræ, on the 31st of August, which is one of the two which differ most from the mean; the others are of *α* Lyræ, August 29th; *α* Cygni, August 31st; and *α* Gruis, September 2nd. I have also found the original noting of the observation of *α* Lyræ, at Bahia, on the 26th of July 1822, by which I am enabled to correct the signs in the 7th, 8th, 10th, and 11th repetitions (Pendulum Experiments, page 298), which have been incorrectly transcribed − instead of +; the summing up of the column (−5) which appears as the mean reading of the level is thus seen to be correct, and the result of the observation recalculated with 10·9, is 12° 59′ 17″, instead of 12° 59′ 21″, as calculated with single seconds.

The original papers to which I have referred are now in Capt. Kater's possession.

2. When transits are observed with a chronometer, and irregularities of less amount than a beat of four-tenths of a second are not taken into account in the registry, the times of passage from wire to wire will be identical in a much greater number of instances than when the actual time of transit at each wire is estimated to the tenth of a second, and registered accordingly, as is the case in observatories; but the latter is without doubt the more exact method of obtaining the absolute time of transit. My purpose, which was simply to determine the rate of a clock, did not require that I should adopt the best method of observing absolute transits, but merely that the method adopted at the commencement should be also employed at the close.

3. The purpose for which naval officers observe Lunars, is usually to ascertain the position of a ship at sea; for that purpose it is not requisite, nor is it customary, to attend to several particulars in respect to the instrument, the modes of observation and calculation, on which precision in the result

sult depends. My object was different; it was to employ Lunars as a means of accurate determination of longitude on shore; and with that view I neglected nothing that could conduce to accuracy, either in the observation or in the result. I may add, that there can be few naval officers who have had so much experience, in that kind of Lunar observation, as I had already gained before I commenced the observations in 1822, having before that time published the results of more than two thousand such observations. Of the seven stations the longitudes of which I determined by Lunars in 1822, one only, so far as I know, has been since examined, and the result published. I made the longitude of the Barrack Square in the Island of Ascension 14° 23′ 46″·5 W., being about ten miles west of the longitude given by Professor Lax, as furnished to him by the Hydrographic Office, and nearly twenty miles west of the longitude assigned in the *Connaissance des Tems;* which two publications might with propriety be regarded as the best authorities of that time. Capt. Duperrey, who visited Ascension in 1825, with pendulums, made the longitude of the Barrack Square, by chronometers from St. Helena, 14° 24′ 05″·7; and from Tarifa 14° 24′ 21″·2, both determinations being within one mile of mine, and both still more to the westward.

4. Those who are conversant with experiments with invariable pendulums know that the accord of partial results is dependent, not on the skill of the observer,—for the method is such as to render the observation nearly independent of individual skill,—but on the quality of the clock in keeping an uniform rate in short intervals, and on the means of preserving an equable temperature. It is difficult to institute a just comparison in these respects between the different circumstances of different observers.

There is one case, however, though it is a rare one, which does permit a comparison; it is when the circumstances of two observers have been exactly the same. The last published part of the Phil. Trans., containing Capt. Ronald's observations, with No. 4 Pendulum in London, affords such an opportunity. Capt. Ronald was furnished with the same invariable pendulum which I had previously employed; he tried it in the same room, and with the same clock that I had used; the circumstances were as nearly the same as possible, except that it was the first time (at least I believe so, for I was not then in London) that Capt. Ronald had observed with an invariable pendulum. Of eleven results obtained by him in London, there are eight of which the difference from the mean does not exceed one-tenth of a vibration in twenty-four hours; and the three others are all within one-fifth of a vibration, per

 diem,

diem, of the mean. These results are quite as accordant as those which I had obtained under the same circumstances. I may remark also, that Capt. Ronald's mean result differs only one-fiftieth of a vibration in twenty-four hours from the mean result of my observations.

Further, if the observations are examined of the many persons, both in this country and elsewhere, in conjunction with whom I have at different times made pendulum experiments, amongst whom are several who, though eminent observers in other respects, were unaccustomed previously to that kind of observation,—abundant evidence is afforded, that the circumstances being the same, the accord of the partial experiments is nearly the same, whether the observer be previously inexperienced, or whether he has had, as I undoubtedly have, the advantage of very considerable practice.

IX. *Corrected Elements of the Comet lately observed in Pegasus, founded upon Observations made by* Mr. Taylor *at the Royal Observatory; with Observations and Elements of the same Comet, by* M. Valtz, *of Nimes. By* Charles Rumker, *Esq.*

To Richard Taylor, Esq.

Dear Sir,

SINCE your last Number was published, I have had an opportunity of correcting my elements of the late comet in Pegasus, upon the observations made thereon at Greenwich, and communicated to me by Mr. Taylor; and I have now the pleasure of sending you these observations, together with my corrected elements.

Observations made at Greenwich.

	Sidereal Time.	Apparent Right Ascension.	Apparent North Polar Dist.
	h m s	° ′ ″	° ′ ″
1830. May 4	17 45 59·3	21 15 43·4	71 57 18·6
16	17 27 48·3	21 18 21·3	67 21 54·4
17	17 33 41·6	21 18 21·3	67 4 19·4
22	17 31 42·8	21 18 46·0	65 43 1·4
30	18 8 22·2	21 15 13·1	64 2 50·5
June 1	18 9 20·2	21 14 10·7	63 43 42·5

Corrected Parabolic Elements of the Comet.

Time of passage over the perihelion, 11th of April, $2^h\ 7^m\ 30^s$ Mean time at Greenwich.

Longitude

Longitude of the { Perihelion 213° 20′ 47″ from apparent equinox 22nd of April. Ascending node 205° 48′ 17″.

Logarithm of perihelion distance 9·9677241.

Inclination 20° 28′ 31″.

Motion direct.

There appears to be an error in the right ascensions of either the 16th or 17th of May, as the comet has reached its maximum of Æ somewhat later; but Mr. Taylor has found them thus recorded in the Journal.

I take this opportunity of sending you some observations made by Mr. Benjamin Valtz, at Nimes, with the elements calculated by the same gentleman from his own observations.

Observations of the Comet made at Nîmes, by M. Valtz.

1830.	Mean time from Midnight.	Apparent Right Ascension.	North Declination.
	h m s	° ′ ″	° ′ ″
May 5	3 18 20	318 54 30	18 0 2
19	1 27 19	319 35 23	23 13 8
26	0 47 1	319 18 27	24 58 16
30	2 50 5	318 55 32	25 47 32

Elements of the Comet calculated by M. Valtz from his own Observations,

	Until the 19th of May.	Until the 30th of May.
	Days.	Days.
Pass. over perih.	April. 14,382	April. 9,876 mean time from midnight.
Long. of perihelion...	215° 2′	212° 14′
Ascending node......	206 3	206 22
Inclination............	19 47	21 16
Log. perih. dist. ...	9·97313.	9·96454
	Motion direct.	Direct.

The last elements represent the observations of M. Valtz to the nearest minute during a period of 38 days. This comet has passed within $\frac{1}{20}$th of the sun's distance from the earth towards the end of the month of March, and could have therefore been sooner discovered.

At Geneva the comet was seen until the 2nd of June, but showed only an indistinct mass of light, very pale and difficult to be seen; and on account of its great faintness, which could not bear any illumination of the wires, no observations could be made on it. I remain,

Dear Sir, yours truly,

CHAS. RUMKER.

6, Caroline Place, City Road,
June 22, 1830.

X. *Note on* Mr. MacLeay's *Abuse of the Dichotomous Method in Natural History. By the Rev. Dr.* Fleming.

To the Editors of the Philosophical Magazine and Annals.

"Art thou thus bolden'd, man, by thy distress?
Or else a rude despiser of good manners,
That in civility thou seem'st so empty?"

Gentlemen,

YOUR Magazine for June having reached me at the ordinary period, I proceeded to an examination of its contents with the usual degree of interest. The article from the pen of Mr. MacLeay "On the Dying Struggle of the Dichotomous System" naturally attracted my notice, not merely as an attack against myself, but as the exhibition of a mode of conducting philosophical discussion I had never witnessed before. Whether this new style be calculated to advance the interests of science, to increase the respectability of your Journal, or to promote friendship among naturalists, must be left to the decision of the moral feeling of your readers and the public. In the meantime, however, I may take the liberty of stating, that if there be any of your readers capable of relishing such kind of lucubrations, they may blame you for having, hitherto, neglected to gratify their taste; while I assure them that I have no wish to secure their favour.

The subject of "Methods in Natural History" is one of very great importance to the interests of science, though hitherto in this country in a great measure disregarded. Discussions connected with it, and conducted in a suitable manner, could not, in such circumstances, fail to be useful. Had Mr. MacLeay, therefore, confined his attack against me as one who admired the dichotomous method and held quinarianism in derision,—to the merits of the respective systems, he would have received the satisfaction of a candid reply; as I am not aware of having published any opinion which I am afraid to defend, or would be ashamed to modify or abandon with increasing knowledge. But Mr. MacLeay, having laid aside the language of a gentleman, and violated the customary civilities of life, has compelled me, in due regard to my own character, to pass over in silence this effusion of his pen, which is probably without a parallel in the records of science.

As Mr. Vigors has thought proper to appear in connection with the publication in question, I request him to assure his friend at Cuba, that he never was the object of my malice or envy, but that at present he shares largely of my pity.

Before concluding, I beg to assure your scientific readers, that I still adhere to the opinions I formerly expressed in my "Philo-

"Philosophy of Zoology," and more recently in "British Animals," respecting the value of the dichotomous or binary method in natural history. With regard to the opinions advanced in the Quarterly Review, I presume that the Editor and his coadjutor are fully qualified to defend themselves, or rather that they are disposed to smile at the harmless abuse which Mr. MacLeay has thought proper to send forth against them. They are accustomed to witness the "dying struggles" of harpooned whales. It is indeed their pastime. I am, &c.

Manse of Flisk, June 10, 1830. JOHN FLEMING.

XI. *On the Dying Struggle of the Dichotomous System. By* W. S. MACLEAY, *Esq. M.A. F.L.S. In a Letter to* N. A. VIGORS, *Esq. F.R.S.*

[Continued from p. 445.]

[Upon the reconsideration of this article, we cannot but regret, in common with many others who take interest in the discussion, that so much personality should have been introduced into a scientific controversy; and Mr. MacLeay's paper having been printed *entire* for private circulation, we have, in acquiescence with the general opinion, omitted, in the continuation which follows, and which will be concluded in the next number, many paragraphs, &c., irrelevant to the subjects discussed. The portions of the paper, therefore, which our readers have now to peruse, must be considered as consisting only of a series of connected extracts from the original; containing, however, all the arguments advanced respecting the Dichotomous System. Our opinion of the unfairness of the article in the *Quarterly Review*, had been expressed (See Phil. Mag. and Annals, N.S. vol. vii. p. 379.) before Mr. Macleay's paper had been received; but what authority our much esteemed friend has for ascribing it to Dr. Fleming, is wholly unknown to us. Articles in favour of the Dichotomous System have repeatedly appeared in our pages *.—EDITORS.]

I DO not know that Dr. Fleming has ever enlightened the world on the construction or anatomy of any one single animal: all he has published of value he has gathered from books. Now that any man, aware as he must be that the little acquaintance he possesses with Natural History he owes entirely to a perusal of the works of Linnæus, Jussieu, and Cuvier, should not have the modesty to distrust himself when differing from them on so essential a point as unity of plan in thecreation, is most astonishing. But what shall we say to

* See Phil. Mag. vol. lxii. p. 200, 274; vol. lxv. p. 105, 183, 372, 428; vol. lxvi. p. 172.

a writer

a writer that denies the existence of a single natural method? No, says he, I must admit that there are as many natural methods as there are organs. But where there are an infinite number of methods to effect a given object, such as the creation, it is clear that there can be no fixed plan. If the architect of the kirk of Flisk has in its composition huddled Grecian and Gothic, Saracen and Scotch, with every other order of architecture together, it is clear he had no plan, and our worthy minister preaches in the midst of confusion. So also, if the minister of the said kirk be right, must the creation be a mass of confusion, without any fixed plan on the part of the Creator? And yet he ventures to sneer at one of the most distinguished of naturalists, and to cite the following lines with respect to him:

> "But Reason still, unless divinely taught,
> Whate'er she learn, learns nothing as she ought."

He talks of demonstrating that there is no fixed plan in the creation. But how does he do this? Because, forsooth, the locomotive extremities of the horse and cow are represented by hands in man, fins in the whale, and wings in bats and squirrels; because, moreover, teeth may be incisors in one animal and molars in another, and so on!—therefore, animals are susceptible of one natural distribution according to their locomotive organs, and of others according to their teeth, &c. To return to the kirk: Supposing it to be built on the most harmonious and uniform plan of Grecian architecture, and to be another Parthenon, Dr. Fleming may dichotomize it into architraves and not architraves, columns and not columns, friezes and not friezes, or any positives and negatives he pleases:—but will he tell us that, because he chooses so to divide it, therefore the architect had not one plan for the fabric, but as many plans as there are terms in architecture? Yet such is the reasoning by which he conceives himself forced to admit that the Deity had no plan in the creation. Sorry for it, very sorry he is; but he assures us there is no remedy.

But he says, "If unity of method in the creation be admitted, it will in many cases separate groups from other genera with which they are intimately connected;" and he proves this, by the genus *Lepus* in Zoology and *Sambucus* in Botany, as follows: "The hare, in relation to her offspring, exhibits an affinity with the horse and sheep; while the rabbit, in the same relation, claims kindred (as does also the cat) with the opossum and kangaroo." *Ergo*, a natural dichotomous division, according to Dr. Fleming, is into those that at their birth have their eyes open, containing the natural group the hare, the horse, and

and the sheep; and those which have them shut, such as another natural group comprising the rabbit, the cat, the opossum, and the kangaroo!

The Doctor's botany is on a par with his zoology; for says he, "In any arrangement which contemplated plants according as their stems were capable of producing flowers and fruit during many years, or able to produce flowers and fruit once only, (and the distinction is an important one,) these two species, the dwarf and the common elder, would have belonged to different genera and even different orders." This system of vegetables now proposed by Dr. Fleming, is not quite new. I have before heard of plants annual, biennial, perennial, and everlastings. The credit he deserves, however, is the original consideration of it as a natural method of distribution. A similar system depending on their duration must, without doubt, be equally natural for animals, and he will, perhaps, soon publish, with his usual learning, names for the dichotomous subdivision of *Animals immortal*, and of *Animals not immortal*, in the first of which groups he places himself.

In like manner our botanist would *naturally* class one species of willow with arrow-root, because it is monandrous, and another with the ladies'-slipper, because it is diandrous. All methods are equally good, all divisions are the same, and he is there ready with his pen to favour all alike with crack-jaw names. But enough, and more than enough, of the above examples, which according to him *prove* that there is necessarily a rupture of affinities, "when we restrict a genus or species to a single place in our physiological system." If the reader does not think it proved, the Doctor asserts that it is, and that is quite sufficient.

Dr. Fleming has been, I have already said, so far acute as to perceive that in order to make the dichotomous division of nature go down, it was certainly necessary in the first place to deny all unity of plan in the creation. So also was it absolutely necessary to attack the law of continuity, and to deny the Linnæan maxim, "Natura opifex rerum non facit saltus"; for certainly no method takes such prodigious leaps as the dichotomous, which may also be termed, *par excellence*, the leaping one. Dr. Fleming indeed admits that the law exists when the changes of bodies take place with respect to time and space, but says he, "Where is there even the shadow of proof that the most perfect of created beings must previously have gone through all the progressive steps of advancement, or that among created beings there is such a gradual transition from one kind to another as to render it impossible for man to pronounce where the one ends and the other

other begins?" Now Lamarck never made the first assertion as above stated, nor have I ever made the second; and yet both of us acknowledge the law of continuity in natural history.

In the *Horæ Entomologicæ* I state that "Lamarck had unfortunately from a ready perception of affinities been induced to confound *natural order*, by which is meant the actual regularity of disposition which exists in nature, with that *order of formation*, by which is meant the process of it in time," and had thus fallen on that *system of progressive development* which Dr. Fleming now thinks he has so wittily caricatured. Indeed every naturalist who has had any perception of affinities bestowed on him by nature as among created beings, observed a regularity of disposition and a gradual transition from one kind to the other, which, although not such as the Doctor above describes it, is nevertheless certain. No one indeed can doubt the fact. Let Dr. Fleming visit any great museum in Paris or London, let him for once in his life take the scalpel in hand, let him study such books as the *Philosophie Anatomique*, and he will soon be sensible how ignorant he has been of natural history; that is, if there be not some natural imperfection that prevents him from detecting affinities.

True it is, that chasms occur; but now, thanks to our collectors, those chasms are comparatively trifling, and moreover are every day filling up. As to their being many, it may appear paradoxical to the Doctor, but I wish to see infinitely more of them. If indeed those that exist should never be filled up by the exertions of collectors, we may still safely attribute them either to that extinction of species which has manifestly been produced by the ancient revolutions to which the surface of this globe has been at various times exposed, or to that extinction which has been produced by the hand of man. Geology, however, according to our author, is opposed to such bold ideas. Why? "Because the strata present to the student the relics of various groups of organized beings,"—a most convincing argument truly; and, secondly, "Because the fossils of the chalk rocks must not be mingled with those of the carboniferous limestone, nor with the species which now exist. All these must be studied as separate systems"! That is, the shell of a chalk rock is not a shell if it occurs in carboniferous limestone, and still something else if it occurs in Flisk.

"Greatly to our annoyance," says the Doctor, "nature occasionally makes a halt—as when she refused retractile claws to the hunting-tiger"! So that he does not merely lay down "first principles of arrangement founded on abstract reason-

reasoning," but, attempts to support them by examples. Let such men but have the heedlessness to pin themselves down by an example, and the utter futility of their reasoning is manifest at once. He cites the *chittah* for a want that only proves how the genus *Felis* passes off to the Canine tribe. And this he calls a halt! So also he says, "Nature indulges in frolicsome leaps, as in passing from the vertebral to the invertebral animals, and completes the confusion of those who wish to train her, by bolting off the course to convey man to his rational throne." The frolicsome leap from vertebral to invertebral animals I shall hereafter show to be grounded on ignorance of zoology. I shall merely now ask any person, whether naturalist or not. Does nature really bolt off her course in conveying man to the throne of reason? It may be indeed that her paces are not always equal—that I believe to be truly the case; but nevertheless she remains steadily on the course, and if she has put man on the throne, she has also placed a series of animals on the steps that lead up to it. Some persons indeed have doubted, whether those steps on which she has placed the ourang-outang and some savage tribes of man, ought upon the whole to be considered the widest apart; and were it not that man possesses the gift of speech, they would so doubt with reason. There is no occasion, therefore, "to hope for the discovery of a semirational species to fill up the greatest gap that exists."

But, after all, this has really little to do with our present subject, unless Dr. Fleming be a materialist. We are now discussing the forms of matter, and unless Dr. F. thinks that mind is matter, he has no business to bring his reason upon the carpet. In their corporeal structure the ourang-outang and negro differ but little—it is degrading to think how little. It has however pleased the Deity to distinguish man by adding to his body a conscious immaterial being, endowed with a degree of free agency sufficient to render it morally responsible to its Creator. *Βουλευτικον δε μονον ανθρωπος εστι των ζωων· και μνημης μεν και διδαχῆς πολλα κοινωνεῖ, αναμιμνησκεσθαι δε ουδεν αλλο δυναται πλην ανθρωπος.* (*Arist. Hist. Anim.* lib. i. c. 1. Ed. Schneid.) Secondary operative causes are no doubt constructed, like forms of matter, also on a wise plan; but if Dr. Fleming wishes to form a Dichotomous System of them, I fear he must patiently wait for his departure from a world which has furnished us only with senses capable of distinguishing the various forms of matter.

[To be continued.]

XII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Abstracts of papers which have lately been read before the Royal Society:—

"ON the elasticity of threads of glass, with some of the most useful applications of this property to various kinds of Torsion Balances." By William Ritchie, Esq., F.R.S., &c.

The author proposes the employment of threads of glass in the construction of torsion balances, in place of the silver wire used by Coulomb, for the measurement of minute electric or magnetic forces. He describes a galvanometer of his invention acting upon this principle, the intensity of the galvanic current being measured by the torsion of a slender filament of glass, to the lower end of which a magnetized needle is fixed at right angles. He also applies the same power to the improvement of the sensibility of the common balance for weighing minute bodies, by affixing to the beam a long glass thread horizontally in the axis of suspension, by the torsion of which, when the balance has been brought nearly to a level, the more accurate adjustments are to be effected. On the whole he considers that glass, from its perfect elasticity, possesses decided advantages over metallic wires, for the construction of instruments acting on the principle of torsion.

"On the quantities of water afforded by springs at various periods of the year." By J. W. Henwood, Esq., F.G.S. Communicated by the President.

It has been a matter of dispute, whether the whole of the water afforded by such springs as are but little influenced by the change of the seasons was derived from rain. With the hope of elucidating this question, the author endeavours to ascertain the comparative quantities of water yielded by the same spring at different periods; and to obtain simultaneous observations in springs rising in different strata and existing at considerable depths in the earth. For this purpose he has availed himself of the information contained in a paper by the President of the Royal Society, of which an abstract was given in the last number of the Phil. Mag. and Annals, on the performance of steam-engines in the Cornish mines. The details of these investigations occupy several tables. After making due allowance for the loss of water, owing to imperfections in the engine, which he considers as nearly balanced by the amount of rain-water which penetrates from the surface and is carried off by the adit, he thinks himself warranted in assuming the actual quantity of water raised by the engine, as representing with sufficient accuracy that which would be naturally afforded by the springs of the mine. On comparing the known quantity of rain falling in any district with the quantity of water given out by its springs, added to that returned to the atmosphere by evaporation from the same district, which he estimates according to Mr. Daniell's method, he finds the former of these quantities is to the latter nearly in the proportion of two to three. After adverting to the hypothesis of the infiltration of sea-water, which might be proposed in explanation of

of this excess in the supply of springs, he remarks that he was not able to detect the presence of sea-salt in the water from the bottom of the mine of Wheal Towan, which he examined in August 1828.

"On the preserved bodies of aboriginal Peruvian Indians." By Dr. Carter. Communicated by Dr. Granville.

In this paper a description is given of the bodies of a female and of an infant, which were lately found in a state approaching to that of mummies, at the foot of a hill forming a promontory near Arica, on the western coast of Peru, and which were sent to England in 1827, by Dr. Hamett, and are now deposited in the Museum of Natural History at Haslar. A tradition exists that the desolate spot where they were dug up was an ancient burying-ground of the aboriginal inhabitants, although it is certain that no interments have taken place in it since the first invasion of Peru by the Spaniards. The cloth which formed the outer envelope of the mummy is of a dark brown colour, and woven from the wool of the *Camelus vicugna*. The inner covering is of a finer texture, and consists of white cotton, either woven or spun, with blue stripes. The body has been compactly put together, and doubled up in a square form, with the breast upon the knees; the arms folded over the abdomen, and the face depressed, so as to occupy as small a space as possible. It was strongly confined, by several turns, with the *bejuero*, or tough and luxuriant creeping osiers, naturally twisted together, and knotted at regular rhomboidal intervals. Within the case were contained a considerable quantity of leaves of unknown plants remarkable in having lateral nerves, matté, heads of Indian corn, pods of capsicum, and two small globular vases. The skin of the body had the appearance of dried leather; the hair was well preserved, and was collected into long black platted tresses, doubled over the chest. Many of the muscles remain, perfectly exsiccated, but distinctly marked. There was also found in the same place a detached head, apparently that of a female Indian; and from the peculiar care bestowed on its preservation, probably the wife of a cacique. The hair is still glossy, and in good preservation, very black, lank, and coarse, and firmly platted. The brain appears to have been extracted through the occipital foramen, and its place supplied by some bituminous substance, filling the cavity of the cranium. The fillets surrounding the head are terminated by knotted fringes, of differently coloured worsted, constituting the *quissa* of the Peruvians;—a species of symbolical writing not used for oral tradition, and, in this instance, serving as a record of the history of the deceased. This head appears to be much flattened posteriorly, and the frontal bone is also depressed; both of which are well known to be characteristic of the skulls of the aborigines of South America; and which were probably the result of artificial compression applied to the head during infancy. The author then enters into a disquisition respecting the funeral customs of the Indians, their modes of embalming, and of manufacturing cloths for interment. He concludes by a variety of statements illustrating the desiccating influence of the atmosphere and soil in those regions, by which the bodies of men and animals are preserved in a dry state, somewhat analogous to that of the Egyptian mummies, for a very considerable number of years.

 "Experiments

"Experiments to determine the quantity of light reflected by plane metallic specula under different angles of incidence, with a description of the photometer made use of." By Richard Potter, Jun. Esq. Communicated by the President.

Sir Isaac Newton has stated, that metallic specula, in common with all other substances, reflect light most copiously when incident most obliquely. Some experiments made by the author, with specula of his own construction, having raised doubts in his mind as to the accuracy of the prevailing opinion on this subject, which accords with that of Newton and of Bouguer, he instituted a more exact inquiry into the proportions of incident and reflected light from specula at various angles of incidence. He used for this purpose a photometer resembling that of Bouguer, and consisting of an upright screen, with a square aperture, across which a piece of thin tissue-paper was extended, destined to receive on one compartment the reflected light from one lamp, and on another compartment the direct light from another lamp, employed as a standard of comparison. By adjusting the respective distances of the lamps, the lights on the paper were rendered sensibly equal in point of intensity,—the equality being judged of by the eye, viewing them from the other side. The measurements were taken alternately, first one of the direct, and then one of the reflected, lights, until a sufficient number of uniform results were obtained. The author, after taking every precaution that occurred to him for ensuring accuracy, invariably found that the proportion of light reflected by metallic surfaces, instead of increasing, diminished in pretty regular gradation, as the angle of incidence was augmented. Thus, in the first experiment, when the angle of incidence was 20°, the proportion of the reflected to the incident light was as 69·45 to 100, at 40° it was 66·79, and at 60° it was reduced to 64·91. Some irregularities occurred in the series of results deduced from different sets of experiments, arising partly from the variableness of the light given out by the lamps, and partly from the difficulty of preserving the metallic surface in the highest state of lustre which it has when newly polished. The author combats the opinion, that the quantities of light which metals are capable of reflecting when polished are in the ratio of their densities; and finds, that in those metals which were the subjects of his experiments, the quantities of light absorbed, or lost by reflection, at incidences nearly perpendicular, are almost exactly in the ratio of their specific heats.

"On the theoretical investigation of the velocity of sound, as corrected from M. Dulong's recent experiments, compared with the results of the observations of Drs. Moll and Van Beck." By Dr. Simons, assistant at the observatory of Utrecht.

Laplace has demonstrated, that Sir Isaac Newton's formula for obtaining the velocity of sound, requires, in order to render it correct, that it be multiplied by a certain co-efficient, depending on the ratio between the specific heats of atmospheric air under a constant pressure and under a constant volume. Laplace has endeavoured to deduce this co-efficient, first from the experiment of MM. de la Roche and Berard; secondly, from those of MM. Clement and Desormes; and lately from the more accurate investigations of MM. Gay-Lussac and Welter.

Welter. By applying this correction, the velocity of sound deduced from calculation corresponded very nearly with the result of actual experiment. Still, however, a degree of discordance was always found to take place. With a view to perfect the theory still further, Dulong attempted, by reversing the process of Laplace, to deduce the co-efficient by which the Newtonian formula is to be multiplied, directly from experiments themselves. The object of the present paper is to compare the investigation of Dulong with the experiments on the velocity of sound made by Drs. Moll and Van Beck, of which an account has lately been published in the Phil. Trans. By applying the values of the co-efficients thus obtained, the computed velocities of sound came out much nearer to the observed velocities; and the author concludes by remarking, that such differences as yet remain between calculation and experiment, may, with great probability, be ascribed to the errors which are unavoidable in observations of so complicated a nature.

"On the occurrence of Iodine and Bromine in certain mineral waters of South Britain." By Charles Daubeny, M.D. F.R.S., Professor of Chemistry in the University of Oxford.

The author lays claim to being the first who announced to the public the existence of bromine in the mineral springs of England: a discovery similar to that which had been previously made by others in many analogous situations on the continent. His reason for offering the present communication to the Royal Society is, that he has examined on the spot a great number of mineral springs, and endeavoured to obtain, wherever it was practicable, an approximation to the proportion which iodine and bromine bear to the other ingredients. He has also aimed at forming an estimate of their comparative frequency and abundance in the several rock-formations; an object of considerable interest in geology, as tending to identify the products of the ancient seas, in their most minute particulars, with those of the present ocean. The results of his inquiries are given in the form of a table, in which the springs, whose waters he examined, are classified according to the geological position of the strata whence they issue, and of which the several columns exhibit the total amount of their saline ingredients; the nature and proportion of each ingredient, as ascertained by former chemists, or by the author himself; and, lastly, where they contained either iodine or bromine the ratio these substances bear to the quantities of water, and likewise to the chlorine also present in the same spring. He finds that the proportion of iodine to chlorine varies in every possible degree; and that even springs which are most strongly impregnated with common salt, are those in which he could not detect the smallest trace of iodine. The same remark, he observes, applies also to bromine; whence he considers, that although these two principles may, perhaps, never be entirely absent where the muriates occur, yet their relative distribution is exceedingly unequal. The author conceives that these analyses will tend to throw some light on the connexion between the chemical constitution of mineral waters and their medicinal qualities. Almost the only two brine springs, properly so called, which have acquired

acquired any reputation as medicinal agents, namely, that of Kreutznach in the Palatinate, and that of Ashby-de-la-Zouch in Leicestershire, contain a much larger proportion than usual of bromine,—a substance, the poisonous quality of which was ascertained by its discoverer, Balard. The author conceives that these two recently found principles exist in mineral waters in combination with hydrogen, forming the hydriodic and hydrobromic acids, neutralized, in all probability, by magnesia, and constituting salts, which are decomposable at a low temperature. He has no doubt that a sufficient supply of bromine might be procured from our English brine springs, should it ever happen that a demand for this new substance were to arise.

LINNÆAN SOCIETY.

June 1.—The President, Lord Stanley, in the chair.

Read, the commencement of a paper by J. O. Westwood, F.L.S., &c. upon the *Paussidæ*, a family of coleopterous insects.

The objects which Mr. Westwood has selected for his communication, form, perhaps, the most interesting family of Beetles, not only from the extremely singular and anomalous structure of some of their most material organs (especially the antennæ, which in some of the insects are larger than the head and thorax together, and composed of only two joints); but also from the circumstance of the typical genus constituting the final entomological labour of the immortal Linnæus, whence, as some authors have imagined, the generic name *Paussus* was derived from the Latin *pausa*, a pause or full stop.

The species are inhabitants of the tropical regions of the old world, and do not exceed half an inch in length.

A paper was read before the Linnæan Society in 1798, upon these insects, by Professor Afzelius, to whom five species only were known. Mr. Westwood has, however, described twenty-three species (besides several others which have been incorrectly placed in the family,) including several new genera.

The following is the "Synopsis Generum" given by Mr. W.

Elytra subquadrata, palpi labiales elongati	Antennæ quasi bi-articulatæ	Caput (ocellis duobus) thorace immersum		3 Hylotorus.
		Caput (ocellis nullis) collo instructum	Palpi labiales articulo ultimo elongato......	2 Paussus.
			Palpi labiales articulis æqualibus........	4 Platyrhopalus.
	Antennæ quasi 10-articulatæ			5 Cerapterus.
	Antennæ quasi 6-articulatæ			1 Pentaplatarthrus.
Elytra subovata, palpi labiales brevissimi				6 Trochoideus.

Gen. 1. *Pentaplatarthrus,* Westw.—A new and very decided genus founded upon one undescribed species, which he names *P. paussoides.*

Gen. 2. *Paussus,* Linn.—Twelve species, four of which are new.

Gen. 3. *Hylotorus,* Dalm.—One species. *P. bucephalus,* Dalm.

Gen. 4. *Platyrhopalus,* Westw.—the type of which is the *Paussus denticornis,* Don. four species, two of which are new.

Gen. 5. *Cerap-*

Gen. 5. *Cerapterus*, Swed.—Three species, one of which is supposed to be new.

Gen. 6. *Trochoideus*, Westw. — One species. *P. cruciatus*, Dalm. This interesting insect was found by Dalman, in copal gum.

Mr. Westwood also mentions the *Hispa bihamata* of Linnæus as supposed to belong to the family, and has also given the characters of a new genus, which he names *Megadeuterus*, related to the Telephoridæ, containing two species, the type of which is the *Paussus flavicornis*, Fab. The drawings exhibited in illustration of the paper, comprised fifty-five figures of species and their anatomical details, including representations of all the genera and new species described by the author.

A paper by John Morgan, Esq. F.L.S. was read, in which the author described some anatomical peculiarities which he had met with in the organs of deglutition in several of the order Rodentia.

It appears that in the Capybara (*Hydrochærus Capybara*), as well as in some of its congeners, Mr. Morgan has found a singular development of the velum palati, or membrane interposed between the mouth and throat, to which he has assigned functions different from those which are attributed to this structure in any other class of animals. After noticing the very extensive grinding surfaces of the molar teeth of the Capybara, and the necessity for such an arrangement of parts in the masticating organs of an animal living occasionally upon hard vegetable substances, and possessing a simple stomach, as in the case of this species, the author proceeded to show that the complete mastication of the food is not only provided for by the form and extent of the teeth, but that it is rendered absolutely indispensable to the passage of nutriment from the mouth to the stomach, by the peculiar conformation of the velum palati, which occupying the whole area of the passage through the fauces, would form a complete septum between the mouth and pharynx, but for the existence of a small circular aperture in its centre, through which the food is allowed to pass. The velum palati thus formed assumes the shape of a cone or funnel, during the act of swallowing, from the pressure of the food against its anterior surface, and the smaller end or apex of this funnel, which is terminated by the central aperture, is thrust backwards into the cavity of the pharynx beyond and above the opening of the glottis, to which part it thus offers additional protection. A sort of membranous strainer is thus produced, through the small aperture of which the grosser particles of unmasticated food are prevented passing from the organs of mastication to those of digestion. The muscles attached to these parts were also particularly described.

A paper was also read, entitled " An attempt to introduce a more precise distribution of the genus *Papilio*, by George Milne, F.L.S."

In this paper Mr. Milne describes the methods of distributing the insects belonging to the genus *Papilio*, which have been adopted, successively, by Linnæus, Fabricius*, and Latreille. Mr. Milne then proposes

* The method adopted in the *Systema Glossatorum* of Fabricius, which Mr. Milne alludes to as having never seen, was, we believe, scarcely kown in this country before the sketch of it, with which we were favoured by Mr. Children, appeared in the Phil. Mag. and Annals, N.S. vol. vii. p. 118.

proposes to restore *Papilio* to the rank assigned to it by Linnæus, at the head of the genus. The characters distinguishing the subdivisions, or phalanges, he draws from the anatomy of the wing, and from the general construction, particularly of the posterior wing. He names the phalanges as follows: *Equites, Heliconii, Danai, Nymphales, Satyri, Morphi, Plebeii, Uraniæ.* The paper concludes with some remarks upon the innovations which have been made upon the Linnæan System, confined chiefly to Lepidopterous insects.

GEOLOGICAL SOCIETY.

May 7.—Thomas England, Esq. B.A. of Pembroke College, Cambridge; Howard Elphinstone, Esq. M.A. of Trinity College, Cambridge; and Robert Edmond Grant, M.D. F.R.S. Ed. Professor of Comparative Anatomy and Zoology in the University of London,—were elected Fellows of this Society.

A Paper was read, entitled "Sketches explanatory of Geological Maps of the Archduchy of Austria and of the South of Bavaria;" by Ami Boué, M.D. For. Mem. G.S. &c.

The accompanying maps of the Archduchy of Austria and of Bavaria were made during repeated visits to those countries, and partly with the assistance of M. Partsch of Vienna.

The author premises that in consequence of his last visit in 1829 he has changed some classifications, and rectified certain errors which appear in his former works.

I. *Structure of the Archduchy of Austria.*—Dr. Boué describes the principal part of Austria as consisting of the primary chain of Southern Bohemia on the north, and of the great secondary calcareous Alpine chain on the south, which are separated from each other by the tertiary and alluvial valley of the Danube. He divides this last region into three parts:—

1. The molasse and alluvial basin of Upper Austria, extending from Bavaria to near Blindenmarkt and St. Leonhard.

2. The basin of St. Polten, containing shelly sand, sandstone, marl, alluvial marl, and gravel.

3. The basin of Vienna, which is now united with that of St. Polten by a narrow gorge of the Danube.

The direction of the primary chain of Bohemia is from south-west to north-east; gneiss being the predominant rock, with some subordinate masses of granular limestone and diorite. Granite occurs in the western, and sienite, leptinite, and serpentine in the eastern part of this range. The central ridges of the Alps are primary, and these are succeeded, in an ascending order, by talco-quartzose rocks, distinguished by masses of compact limestone with iron ore. Between the preceding rocks and the escarpments of the Alpine limestone, are ancient longitudinal valleys, which certain rivers occupy in their early course, and afterwards quitting abruptly, run at right angles through newer and transverse rents in the secondary formations. At the base of the Alpine limestone, and subordinate to it, are red sandstone and shale, with gypsum, but without porphyry. This group can be traced from Mont Blanc to Hungary, and it again appears in

in the Tatra, or northern Carpathians. The Alpine limestone is characterized generally by organic remains common to the superior secondary formations, such as belemnites, ammonites, nautili, echini, and many zoophytes; but accurate subdivisions of it are made with great difficulty.

One of the most important of these subdivisions is marked by the presence of salt and gypsum, which are found in shale, associated with gray sandstone and limestone, containing belemnites, ammonites and fuci; and in some places, as at Hallein, with orthoceratites and madreporic limestone.

Dolomite prevails in the upper part of the Alpine limestone, and is usually connected with peculiar anomalies of stratification and inclination, which according to the author offer evidence of the rupture and friction of the displaced masses; the whole having, he conceives, been elevated and depressed by the action of subterranean gaseous forces.

Another member of the Alpine limestone is characterized by lead and iron ores.

The Alpine limestone passes into a superior sandstone (designated as "Vienna sandstone"), with alternations of marl and schistose lithographic limestone and whetstones. This part of the series contains coal at Greater Ipsitz, &c. with cycadeæ; and in other places this group is capped by ruiniform, compact limestone, with ammonites, belemnites, and fucoides. (St. Veit, Sontagsberg, Elixhausen, &c.)

Serpentine and greenstone traverse secondary sandstone at Ipsitz, and both sandstone and Alpine limestone at Willendorff.

The author then proceeds to identify certain rocks having a similar mineralogical character, whether in the northern Carpathians, where they rest upon the "Vienna sandstone", or at Grünbach near Vienna, where they are stated to contain belemnites and Ananchytes ovata, with the formations of Gosau-thal, which Messrs. Sedgwick and Murchison, he states, have erroneously described as tertiary. He does not admit that this deposit of Gosau can be considered as intermediary between the secondary and tertiary formations, but he assigns to it the place of the lowest secondary green-sand.

The tertiary character of many of the remains is not considered by him to prove the age of this deposit, for he states that some fossils in the oldest secondary rocks at Hale, Bleiberg, and Maibel in Carinthia, have also a tertiary appearance.

The true green-sand of the Alps is then described; and the author identifies the iron ores of Sonthofen with those of the Kressemberg, which Count Munster, as well as Messrs. Sedgwick and Murchison, has considered tertiary*.

Chalk is stated not to exist in the German Alps, though the lower green-sand of Gosau contains beds like the Planer Kalk or upper green-

* In this part of the paper Dr. Boué has been led into an error in consequence of misunderstanding a passage in the abstract of a communication by Messrs. Sedgwick and Murchison, published in the Phil. Mag. and Annals for January 1830, p. 53. The deposit of Sonthofen was never considered tertiary; but on the contrary, was distinctly stated by them to be secondary.

 sand.

sand. The tertiary deposits of Austria are stated to belong entirely to the superior division of that great class of rocks, and the author asserts that they in no case enter into the Alpine regions, except on the eastern side, viz. in the drainage of the Mur, the Scive, and the Drave, where they occupy ancient longitudinal valleys.

Häring, described by Messrs. Sedgwick and Murchison as an ancient estuary, or area of the great tertiary sea of Bavaria, is considered by the author to be a continental local freshwater formation.

The lowest tertiary formations of Austria are, he says, characterized by blue, shelly marl, and marly, shelly molasse (Schlier), which he assimilates to sub-apennine marl.

In lower Austria this blue marl is succeeded by sands, marls, lignite, and shells, both marine and fluviatile, and these again by gravel and conglomerate, and lastly by nummulite and coralline limestone, alternating with sands and conglomerates, which separate the true tertiary basins of Vienna and Hungary from the deposits of the alluvial period.

The oldest alluvial gravel follows many Alpine valleys in the form of terraces, and the same is extended with beds of marl far into the actual valleys of the Danube and the March, and also into the plains of Hungary, where bones of extinct quadrupeds and terrestrial shells are found in it.

It is in the marl of this old alluvium near Krems that the human skulls have been found, which have been described by Count Breunner. The author remarks on the peculiar form of these skulls, and their resemblance to those of the Caribs and Chilians, &c.; also that he has himself found human skulls in alluvial marl of the same age at Lahr in the valley of the Rhine.

II. *Structure of the south of Bavaria.*—The south of Bavaria is chiefly occupied by an extensive tertiary basin, from 1600 to 2000 feet above the level of the sea, which is bounded by the primary range of Bohemia and the German Jura on the north, by the Alpine chain on the south; whilst it communicates with the tertiary deposits of Vienna and Hungary by the valley of the Danube, and with the molasse of Switzerland on the west.

The German Jura offers no fissures or transverse valleys by which this basin of Bavaria could have communicated with the Neckar and the Maine; and at the period of the tertiary deposits this great depression must have been equally shut out from all communication with the Mediterranean, by the intervention of the Alpine chain, which the author, differing from M. Von Buch, has in former memoirs demonstrated to have been elevated at various periods; an idea which has subsequently been adopted and enlarged upon by M. Elie de Beaumont.

The German Jura contains also the subdivisions of the oolitic series, from lias up to Stonesfield slate and cornbrash, viz.—1. Lias without the white beds. 2. Lias marl. 3. Lias sandstone. 4. Inferior oolite, with iron ores. 5. Great oolite, mostly compact. 6. Dolomitic limestone. 7. Calcareous slate of Solenhofen, with tortoises, fishes, crustacea, sepiæ, ammonites, belemnites, lepadites, insects, and vegetables. Upon

Upon this system of Jura limestone there are small patches of iron and green-sand at Ratisbon and elsewhere. In this deposit, associated with argillaceous marl, are found the pisiform iron ores, or Bohnerz of the Germans, concretionary masses of siliceo-calcareous millstone, with many univalve shells and corals (Natheim), and beautifully zoned, chalcedonic nodules, or *kugel jaspis*, with echini and microscopic shells (near Basel).

The author agrees with Mr. Schübler that it is essential to distinguish this deposit of Bohnerz from those alluvial accumulations with iron ore made up of the detritus of older rocks, and in which are found the bones of many extinct quadrupeds. (Kanderu, Haiberg near Tuttlingen, &c.)

The Alpine chain south of the tertiary basin of Bavaria is constituted of materials nearly the same as in its range through Austria; viz. 1. A base of red-sandstone and conglomerate. 2. Lower limestone with fishes (Seefeld). 3. Gray sandstone and shale with salt and gypsum. 4. Gray dolomite and oolite. 5. Sandstone of Vienna, which though thin and obscure at Salzburg and Sonthofen, expands into a vast formation in its westward range into the Voralberg. 6. Green-sand, filling cavities in the Vienna sandstone, from which it is separated by conglomerates made up partly of Alpine limestone, but chiefly of primary rocks, which are not found *in situ* nearer than the Black Forest. The author conceives this conglomerate to be of the same age as those at the base of the Gosau formations, and in the Allgau; and he further identifies with it the Nagelflüh of Switzerland, which, although hitherto considered tertiary, he places in the lower green-sand; and as proofs of this he cites the existence of a similar conglomerate or Nagelflüh on the summit of the Voisons near Geneva, and also near Saanen, where it overlies and is united with what he considers to be the equivalent of the Vienna sandstone.

The green-sand of the Allgau consists of marls and calcareous sands of various colours containing plants, with here and there subordinate masses of true green-sand, having some characteristic fossil shells of that formation, and iron ore.

For the details of the tertiary rocks of Bavaria the author refers to his last work (Geologische Gemälde von Deutschland). In speaking of the vast alluvial accumulations which encumber this basin, he remarks that the debris are all primary near the primary chain of Bohemia, and secondary on the flanks of the Alps or Jura. Erratic boulders of large size are spread out in lines, and extend to some distance in front of the mouth of the valley of the Rhine; whilst lesser detritus only is found at the debouchure of the Inn. According to the author the elevatory forces which so greatly affected the western Alps must have operated less powerfully upon the eastern prolongation of these mountains.

Alluvial marl, as in Austria, covers the sides of the Danube in its course through Bavaria; and all the lower regions of the latter country offer innumerable proofs of various changes during the alluvial period in the successive drainage of lakes, and in the alteration of the course of rivers.

FRIDAY-EVENING PROCEEDINGS AT THE ROYAL INSTITUTION OF GREAT BRITAIN.

March 26.—Mr. Brooke on the principles of the doctrine of life contingencies. Mr. Brooke developed the manner in which from time to time the probabilities of life had been deduced by the Government and by Life-assurance companies, explaining his statements by reference to tables and to many curious and singular facts in the philosophy of population.

April 2.—Mr. Ainger on the theory of the radiation of heat. Mr. Ainger's point was to explain a difficulty in M. Prevost's theory of the radiation of heat, dependent upon the manner in which surrounding objects frequently influenced the apparent quantity of radiation from a given central heated body, as a thermometer, in complicated and frequently unperceived ways. By taking into account the temperature of all these bodies, and the reflective as well as radiating power of such as were important, he showed that the difficulty really had no existence. He pointed out also by an experiment, that a given body with a constant temperature and surface might appear either hot or cold to the same thermometer according to the temperature of the surfaces, the rays from which would be intercepted by the body and prevented from impingeing on the thermometer.

There were no meetings in Passion and Easter weeks.

April 23.—Mr. Faraday on the flowing of sand under pressure. This was an experimental account of the very curious experiments made by M. Huber Burnand on the intermediate properties which sand exhibited between those of solid and fluid bodies. Sand prepared so as to be uniform and free from dust will flow in the air at angles above 30 or 32 degrees, but not at smaller angles. Sand put into a box or reservoir and allowed to flow out at an aperture, either in the bottom or side, amounts to the same quantity passed, whatever the head of sand may be, or whatever the pressure there exerted, being in this respect quite unlike fluid; so that perhaps it may be made to constitute a moving force probably more independent of deranging causes than any other which can be devised. When a perpendicular tube is filled with sand, very little of the weight is borne by the bottom of the tube; indeed only so much as would equal the weight of a cone of sand standing on that bottom; but nearly the whole is supported by the sides. If a tube an inch in diameter be filled for about six inches or more with sand, and laid horizontally, all attempts to push the sand out of the tube by a stick of nearly the same diameter will fail. These and many more curious facts, with their general principles and applications, were explained and illustrated.

April 30.—Dr. Clarke on the ascent and descent of Mont Blanc. Dr. Clarke had on a former evening given an account of his ascent up Mont Blanc; he now concluded his account, and went at considerable length into the geology and botany of the mountain. His observations were illustrated by numerous specimens of minerals and plants, and also by drawings.

May 7.—Mr. Faraday gave an account of the triangulation which has been proceeding in Ireland for some years past, under the direction

tion of Colonel Colby; and also of the measurement of a base in the North of Ireland, upwards of ten miles in length. This base has been measured by a set of beautiful compensation bars, devised by Col. Colby, and executed in the most perfect manner by Mr. Troughton. Two metals are used in their construction, which support cross bars at their extremities, upon which dots are marked that never change in their distance from each other, whatever change of temperature the apparatus itself undergoes. These dots are observed by microscopes themselves supported on compensation systems. The beauty and perfection of the apparatus, the minuteness of work, and the accuracy of performance, were illustrated by two bars, out of a set of six, which are in course of construction by Messrs. Troughton and Sims, under the orders of the East India Company, and are to be placed in the hands of Capt. Everest for the purpose of an exact survey in the East. Mr. Faraday drew his instruction this evening from Capt. Drummond and Lieut. Portlock, two officers who have been extensively engaged in the Irish measurement.

May 14. —Mr. Burnett on the operation of lithotrity, with the history of the discoveries of Le Roy, Civiale, Heurteloupe, &c. &c. Baron Heurteloupe's apparatus was upon the table, and also that of M. Civiale; and the general principles of the operation were shown, in various ways, upon different calculi. Several cases were adduced to show the value of this great discovery in modern surgical practice.

May 21.—Mr. Faraday on the application of a new principle in the construction of musical instruments. The principle here referred to is that which has lately been so popular in the small musical instruments called Æolinas. A spring generally in the form of a parallelogram being fastened at one end to a plate with an aperture of corresponding size, so as nearly to fill the latter, is put into isochronous vibration when the breath is urged past it, and produces musical sound. The laws of the vibrations of rods and springs were referred to, and then all the instruments which have been constructed on this principle, from the ancient Chinese organ to Mr. Day's Æolian organ, were produced and explained: amongst them were of course the Æolina and Mr. Wheatstone's orchestrion, the fingering and powers of which were fully explained. Each instrument had its capabilities exhibited by performances. Mr. Wheatstone supplied the philosophy of the evening.

May 28.—Capt. Manby on the means of preserving lives in cases of shipwreck, and on a new practical mode of hauling life-boats, &c. through the surf. Capt. Manby gave an account of his own apparatus for these important purposes, and illustrated it by experiments and drawings. That part which related to the preservation of shipwrecked mariners is already before the public; but his method of hauling a boat off from shore is new, and is said to have been found useful. An anchor with a buoy to it is laid out at any previous time beyond the line of surf, a block is fastened to the buoy so that it cannot turn, a rope is passed through this block and both ends brought on shore; but to prevent the rope sinking into the sand, a small buoy is made fast to

to one part of the rope half way between the anchor and the shore; this buoy carries a loop, and through that loop the other part of the rope passes. A boat made fast to one end of this rope is easily hauled through the surf, and can then proceed to give assistance to a ship in distress.

June 4.—Mr. Brockedon on the perception and application of colour. Mr. Brockedon's observations related to the manner in which the eye perceived colours, with their agreement and disagreement, and to some of the compensating forces appointed by nature to prevent distress to that organ when under the influence of bright colours. After remarks upon the complementary colours, with apparatus and diagrams illustrating their principal properties, he proceeded to consider the nature of ocular spectra, which are always complementary to the colours that occasion them. This he seemed inclined to consider as the result of a natural effort in the eye to relieve itself from the undivided influence of any one bright colour; and he referred to the beautiful structure of minute parallel lines, observed and figured by Mr. Bauer, in his drawings of the part immediately over the retina. These lines are governed by muscles which can alter their distance; and thus if the analogy between them and the colours of mother-of-pearl or Barton's engraved plates may be admitted, allow of alterations, which, excited by the influence of coloured rays entering the eye, may cause the appearance of the complementary spectra.

Mr. Brockedon advanced these suggestions very modestly, and only as he said to excite others to inquiry.

June 11.—On the laws of coexisting vibrations of strings and rods. This was one of the series of evening communications, given at this Institution by Mr. Faraday, the matter and illustrations of which are supplied by Mr. Wheatstone. The separate vibrations of a string as a whole, or as subdivided by nodal points, were first shown, and then the coexistence of two or more modes of vibrations in the same string, and the consequent form of the string in different parts of its vibration, illustrated by diagrams. Then the manner in which rods vibrated either in the lowest or in any higher mode was shown, and reference here made again to the coexisting vibrations, and to their combination also with motion of a more general and ordinary kind.

The experiments of Dr. Young were then referred to, in which he observed the figure of the orbit described by any part of a vibratory string, by remarking the line described by light reflected from it, after which the Kaleidophone of Mr. Wheatstone was resorted to for further illustration of these effects. This instrument consists essentially of an elastic steel wire, twelve or fourteen inches long, fixed firmly in a vice or foot at one end, and at the other furnished with a round metallic bead. This serves as a convex mirror, and if held in the sun's rays or near a single light, as a lamp or candle, reflects a spot to the eye. When the wire is made to move, the spot describes an orbit of various shapes according to the path of the end of the wire; and if vibrations be inflicted on the wire, either by tapping it with the finger or by drawing a violin-bow along it, these are rendered visible,

ble, by the forms which they impart to the orbit. The application of this instrument to the demonstration of coexisting vibrations was then made, in many ways.

Mr. Faraday also referred to certain new and curious forms observed when the eye has a motion given to it, either perpendicular to or across the path of the vibrations; a compound result of the motion of the eye with the motion of the vibrations, or moving particles is obtained, which is more or less oblique according to the ratio of the velocities of the two motions. It is expected that extremely rapid motions, defying all ordinary examination, may in this way be measured; but as the subject is at present under consideration, it will be returned to very shortly.

The meeting then adjourned for the season.

SOCIETY OF GENERAL PRACTITIONERS IN MEDICINE AND SURGERY.

The following Prospectus of a plan has been circulated of a "Metropolitan Society of General Practitioners in Medicine and Surgery," signed William Gaitskell, President.

"While almost all public bodies, whether professional or commercial, form associations, corporations, or companies for the purposes of legislating for their mutual protection and for the advancement of their prosperity, it is found that no association of the numerous class of medical men comprehended under the term General Practitioners, has yet in any manner been formed for the protection of their particular interests.

"Various branches of the medical profession have colleges, charters, and corporations, from which the General Practitioner is either altogether excluded, or attached as an appendage only; he is not admitted to a participation in their councils, or to share in their honours; as a General Practitioner he belongs exclusively to neither branch, and is therefore virtually excluded from all.

"A Society has therefore been formed, entitled, "The Metropolitan Society of General Practitioners in Medicine and Surgery," which is intended as an union of the Practitioners of this class throughout England and Wales, for the protection of their mutual and individual interests; having the following objects:—

"1st.—Such alteration of existing laws and customs as shall promote the prosperity and respectability of the general body of Practitioners.

"2nd.—The adoption of such measures as may be conducive to the advancement of medical science and of professional information.

"3rd.—The periodical assembling of the members for literary and scientific discussion—for the cultivation of social intercourse, and for the consideration of general measures relative to the Society.

"4th.—The creation of a fund to be appropriated to the protection of the Members, and for the general exigencies of the Society.

"5th.—The establishment of a Benevolent Fund, by contributions from Members of the Profession at large and other charitable persons, for the relief of distressed medical men and their families.

"The

"The foregoing is a brief statement of the views of the Founders of this Society, and of the advantages intended by its institution, the plan of which may be enlarged, or curtailed, according to the support it may receive."

XIII. *Intelligence and Miscellaneous Articles.*

NOTES ON A LETTER ADDRESSED BY THE SECRETARY OF THE ROYAL SOCIETY TO THE PRESIDENT. BY C. BABBAGE, ESQ. F.R.S.

"THE Secretary of the Royal Society, in a letter addressed to the President *, proposes to refute a charge I have made against the Royal Society in my late work 'On the Decline of Science in England.'

"If the facts stated in that book are correct, explanation is difficult, refutation impossible.

"The case is shortly this:

"The Secretary either did write down the rough minutes of its proceedings during the meeting of the Council on the 26th November, 1829, or he did not.

"If he did not, they must have been written from memory. Is any argument necessary against such an unheard-of course?

"If the Secretary wrote the rough minutes during the sitting, he MUST have written down the name of Captain Beaufort, which he admits was the one decided upon; and it was impossible that he could know of Captain Beaufort's refusal until late the next day, because that gentleman did not communicate to the President his intention of declining the nomination until that time.

"The paper preserved amongst our records which professes to be the rough minute of the Council of the 26th November, is a single folio half-sheet, written on both sides; towards the middle of the second page are the names of the persons recommended. If that of Captain Beaufort were found in this list with a line drawn through it by the pen, and that of Sir John Franklin substituted, it might have been argued that although the subsequent alteration of a rough minute was highly irregular, yet it could give no just ground to suppose that it was intended to conceal from the Society the fact of the refusal.

"But any member of the Royal Society may satisfy himself that the name of Captain Beaufort has *never* been written on that paper. I confess I can see no alternative but to suppose, either that the paper containing those minutes was written from memory *subsequently* to the date it bears—or that it is *not* a genuine document.

"The Secretary has accused me of 'not having chosen to take the slightest pains to inquire into the truth of an accusation,' and also with having drawn the 'sweeping conclusion that the whole of the minutes are unworthy of the least confidence.' To the last of these charges I can only say that I shall be obliged by his pointing out the passage in which it occurs. To refute the first, I shall adduce an evidence which the Secretary will be the last to dispute;—Dr. Roget has

* See our last Number, p. 446.—EDIT.

himself

himself undertaken to assure the world that 'I have spent an immense time in ransacking the records of the Society, with an industry worthy of a better cause.'

"I perfectly agree with the Secretary, that it is becoming in those who bring forward charges, to attend to the accuracy even of the most trifling circumstances which are connected with them; and now that the minutes of the Councils (of the dates to which he has referred) *are entered*, I perceive that there is no entry of any Council held on the 11th of February. That officer possessed better opportunities than myself of knowing the fact that a *Council was summoned for that day* by order of the President, and that it *did meet* in consequence of that summons. The Secretary is therefore inaccurate when he states that 'no Council was held on that day.' And the circumstance of his having neglected to enter its meeting in the Council book, and that the only business it transacted probably was to resolve itself immediately into a Committee of Papers, cannot prevent a meeting regularly summoned from having been a Council; nor if it could, ought it to be urged, at least by *him*, as a reproach to me, had I fallen into a mistake. In fact, as on the 16th of March, the minutes of the Council of the 4th of February and 11th of March had not been entered, I could only know of the existence of a Council on the 11th of February, by inquiring of the Assistant Secretary if he had been directed to summon one, to which he replied in the affirmative.

"Whether these are 'the only instances of inaccuracy in the minutes' I have been *able* to adduce, can scarcely be within the knowledge of the Secretary; time may enable him to correct that opinion. Whatever value may be attached to the explanation he has given, the accuracy of my facts are fully admitted by all: indeed, in the statement made by the President at one of the ordinary meetings of the Royal Society, he not only fully admitted their correctness, but most candidly added that it was quite natural that, in my ignorance of the explanation he was then about to offer, I should have viewed the subject as I had done.

Dorset Street, Manchester Square, June 17, 1830. C. Babbage."

OBSERVATIONS BY DR. ROGET, IN REPLY TO MR. BABBAGE.

"Mr. Babbage has printed a Paper in reply to my refutation of his charges against me, upon which I beg to make a few observations. A slight attention to the arguments contained in the first part of it will show, that they all proceed upon the gratuitous assumption that the Paper which I gave to the Assistant Secretary, to copy into the Minute-book, and which Mr. Babbage says 'professes to be the rough minutes," is the identical Paper written at the Council on the 26th of November. This it neither is, nor ever professed to be. Every one conversant with business must know, that it is generally impossible, during the time of a meeting, to take down more than the heads of what is to form the minutes; and that on many occasions it is requisite that the minutes, in order that they may accurately express the sense of the meeting, should be

be afterwards written out more fully and more deliberately. At the meeting in question, a rough minute was of course taken down; and it did contain the name of Captain Beaufort. That minute was afterwards corrected, as I have already sufficiently explained in my letter to the President. The rough draft itself, being then of no use, was destroyed. The Paper which the Assistant Secretary has in his possession, and which Mr. Babbage has mistaken for the original rough draft, is the fair copy of this corrected minute. The whole of his reasoning built upon this erroneous supposition falls, therefore, to the ground. Had he, both in this and in other instances, taken the trouble to inquire into the truth of facts from the persons capable of giving him the most accurate information, namely from the Secretaries themselves, he would have escaped committing this error, and been spared the pain he must surely feel at having brought forwards groundless accusations: nor would he have hazarded the insinuation, that it was intended to conceal the fact of Capt. Beaufort's refusal to be nominated on the Council; than which, it is scarcely necessary to say, no charge can be more void of foundation.

"The same direct course of inquiry would also have prevented the incorrect statements contained in the latter part of his reply. Had it occurred to him that the persons most likely to give him accurate information respecting the meeting of the Committee of the 11th of February were those who were present at it, he might have saved himself the trouble of reiterating his imaginary charges, and of accumulating hypotheses to sustain them. Instead of assuming that the Council must necessarily have met, merely because it had been summoned; and that having met, it must have transacted business; and that 'the only business it transacted probably was to resolve itself immediately into a Committee of Papers;' and that a minute of such transaction ought to have been made; and that, consequently, the Secretary must have been guilty of negligence in not entering such minute;—he would have learned the real fact, that *no meeting of the Council was held at all.* The President stated at the time, that as there was no business to come before the Council, he had given orders to summon a Committee only, and not a Council; but a summons for the latter had, by accident, accompanied that for the Committee. The Council therefore held no meeting;—the mace was not placed upon the table;—there was no business transacted,—no resolving of itself into a Committee,—no minute to take down,—no negligence in not recording a non-entity.

"As to Mr. Babbage's question respecting the particular passage in his book in which he has stigmatized the minutes as unworthy of confidence, I should indeed be most happy to find that he did not intend to convey the harsh censure which the tone of his criticisms appears to imply. I do not however see how the concluding paragraph of chap. iv. sect. 3. (page 65) can bear any other interpretation.

Bernard Street, June 21, 1830. P. M. Roget."

CHEMICAL EXAMINATION OF WAD. BY DR. TURNER.

Under the name of Wad, or Black Wad, are comprehended several minerals, which are distinguished by the following characters:—They are soft, light, and porous, more or less earthy in appearance, of a brown colour, soil by contact, and contain manganese. Though they agree in these general points of resemblance, several of them are distinguishable from each other by physical properties, and differ essentially in chemical constitution.

First species. Wad from Upton Pyne in Devonshire.

For this Wad I am indebted to the kindness of Mr. König of the British Museum. It occurs in a curved tabular mass about half an inch thick, and may be easily separated into thinner laminæ. It is easily broken, is considerably softer than gypsum, and soils when handled. Its colour is brown with a shade of yellow, somewhat like that of bismuth. The lustre of a fresh surface is considerable, and rather metallic. The streak is brown and shining. It consists of small scaly particles, arranged together so as to give to a broken surface a fibrous appearance. It is very porous, and emits numerous air-bells with a hissing noise when put into water. Its specific gravity, after being boiled in water, is 2·314.

100 parts of the mineral were resolved into

Red oxide of manganese	79·12
Oxygen	8·82
Water	10·66
Baryta	1·40
	100·00

The essential ingredient of the mineral, inferred from these numbers, appears to be a hydrated peroxide of manganese, consisting of 88 parts or two equivalents of peroxide, and 9 parts or one equivalent of water,—a compound which, to my knowledge, has not been observed in the mineral kingdom. Were such a compound quite pure, the analysis should have given the following proportions:—Red oxide 79·12, oxygen 10·57, and water 9;—that is, rather less water and rather more oxygen than was actually obtained.

The hydrated peroxide of manganese may be regarded as the essential ingredient of the Devonshire Wad, and, according to my observation, is the most frequent variety of this mineral. I have not met with it in a state of perfect purity. It usually contains small quantities of some other oxide of manganese, together with baryta, oxide of iron, lime, and silica.

Second species. Wad from Derbyshire.

This Wad, for which I am indebted to Mr. König, is earthy without the slightest crystalline appearance. It acquires a slight lustre by friction, but is otherwise dull. It is very soft and friable, and soils when handled. Its streak and powder are of a reddish-brown colour. It absorbs moisture greedily on being wetted, and when put into water emits numerous globules of air with a hissing noise. Its specific gravity, after its contained air is expelled, is 3·024. It separates readily into parallel layers, the natural joinings being formed by thin

strata of hydrated peroxide of iron, which is largely and intimately mixed with the wad, so as not to be separable from it.

According to analysis, 100 parts of the Derbyshire Wad were resolved into

Peroxide of iron	52·34
Deutoxide of manganese	38·59
Water	10·29
Baryta	5·40
Insoluble earthy matter	2·74
	99·36

The Wad from the Harz, of which Klaproth has given an analysis in the 3rd volume of his *Contributions*, appears to have been of the same nature as the preceding; but it contained a greater proportional quantity of manganese and baryta.

Third species of Wad.

Another species of Wad, of the exact locality of which I am ignorant, was lately sent me from Germany, under the name of *ochreous Wad*, by Professor Hausmann. It is a friable earthy substance, like the foregoing species; but the colour of its streak and powder is dark or blackish brown. It is very porous, and emits a copious stream of air-bells when put into water. Its specific gravity is 4·506.

On exposure to a red heat, after being dried at a temperature of 212° F., it loses 3·08 per cent. of water, together with oxygen gas. Its loss at a white heat amounted to 12·755 per cent.; namely, 3·08 of water, and 9·675 of oxygen. In muriatic acid it is readily dissolved with free disengagement of chlorine, leaving merely traces of insoluble matter. The solution was free from lime and iron, but contained a trace of baryta. Considering its high specific gravity, the small quantity of combined water, and the large quantity of oxygen, which it loses at a white heat, there cannot be a doubt that this species of wad consists essentially of the anhydrous peroxide of manganese, with which a small quantity of some hydrated oxide, probably manganite, is casually intermixed.—*Brewster's Journal.*

ORGANIC TEXTURE OF VEGETABLE FOSSILS.

Mr. Witham, of Lartington, who has for some time been occupied in examining the vegetable remains which occur in our coal-fields and other formations, intends to lay before the public the results of his investigations. A method has been discovered by which the internal structure of these plants may be exposed to view in a most satisfactory manner*, and a series of microscopic drawings made from sections of the fossils is in the course of being engraved. It has been denied that any vegetables of the dicotyledonous class exist in the coal formation, but Mr. Witham hopes to be able to demonstrate the fallacy of this opinion. The plates will be accompanied by remarks on the nature of the plants, and an account of their geological position, and of other circumstances relative to their history.

ACADEMIA CÆSAREA NATURÆ CURIOSORUM.

We have been authorized to state, for the information of the mem-

* See our present Number, p. 20.—Edit.

bers

bers of the *Academia Cæsarea Naturæ Curiosorum*, that the President, Dr. Nees von Esenbeck, has changed his residence from Bonn to Breslaw, where he will continue to conduct the affairs of the Academy. The library has been placed under the charge of the first secretary and librarian, Professor Goldfus at Bonn, in the building appropriated for its reception by the Prussian Government. All communications for the Academy are to be addressed either to Dr. Nees von Esenbeck, or directly to the Academy, at Bonn, or at Breslaw.

MEXICAN MINES.

By the last accounts from Mexico it is learnt that the adit on the great Biscaina Vein at Real del Monte, is cleared to a point where a stream of water coming from the western ground has been intercepted, and instead of falling to the deep parts of the mine, it will now be carried off. It is proposed also to employ the power which may be derived from this water, to drain a part which is expected to be productive, and at present out of the reach of the steam engines. This will be done by one of the hydraulic machines called pressure engines, which it is ascertained can be placed there, and may relieve this part of the mine from water sooner than other means which are pursuing.

LIST OF NEW PATENTS.

To M. Wilson, of Warnford Court, Throgmorton-street, London, merchant, for an improved method of preparing and cleansing paddy or rough rice. Communicated by a foreigner.—Dated the 6th of February 1830.—6 months allowed to enrol specification.

To T. R. Williams, of Nelson-square, Blackfriars Road, Surrey, esquire, for improvements in power looms, applicable to the weaving of wire and other materials —6th of February.—6 months.

To E. Cowper, of Streatham Place, Surrey, gentleman, for certain improvements in the manufacture of gas. Communicated by a foreigner. —12th of February.—6 months.

To J. F. Smith, of Dunstan Hall, Chesterfield, Derbyshire, esquire, for certain improvements in preparing or finishing piece goods, made from wool, silk, or other fibrous materials.—12th of February.—6 months.

To J. M. U. L. R. Du Buisson, of Fenchurch-street, London, merchant, for a new method of extracting, for the purpose of dyeing, the colour from dye woods, and other substances used by dyers. Communicated by a foreigner.—12th of February.—2 months.

To J. Braithwaite and J. Ericsson, New Road, Middlesex, engineers, for an improved method of manufacturing salt.—27th of February.—2 months.

To E. W. Rudder, and R. Martineau, Birmingham, Warwick, cock-founders, for certain improvements in cocks for drawing off liquids.—27th of February.—6 months.

To C. Random Baron de Berenger, Targate Cottage, Kentish Town,

St

St. Pancras, Middlesex, for improvements in fire-arms, and in certain other weapons of defence.—27th of February.—6 months.

To W. Grissenthwaite, esquire, Nottingham, for an improved method of facilitating the draft or propulsion or both of wheeled carriages. —27th of February.—6 months.

To H. Hurst, Leeds, Yorkshire, clothier, for certain improvements in manufacturing woollen cloth.—27th of February.—6 months.

To M. Poole, Lincoln's Inn, gentleman, for a certain combination of or improvements in springs, applicable to carriages, and other purposes.—27th of February.—2 months.

To J. C. Dyer, Manchester, Lancaster, patent card-manufacturer, for certain improvements on, and additions to, machines or machinery for conducting to, and winding upon, spools, bobbins, or barcells, rovings of cotton, flax, wool, or other fibrous substances of the like nature.—27th of February.—6 months.

To W. Grissenthwaite, esquire, Nottingham, for certain improvements in steam-engines.—27th of February.—6 months.

SOLAR SPOTS.

A curious and interesting group of spots are now (May 1st) near the western edge of the sun's disc, in the form of an angle, and there are two large and very black ones in one umbra, just above the angular point. Should they not wear off in going round, they may be seen in the same figure on the eastern side of the sun about the 20th or 21st of May. W. B.

METEOROLOGICAL OBSERVATIONS FOR MAY 1830.

Gosport:—Numerical Results for the Month.

Barom. Max. 30·37. May 16. Wind N.W.—Min. 29·29. May 9. Wind W.
Range of the mercury 1·08.
Mean barometrical pressure for the month .... 29·907
Spaces described by the rising and falling of the mercury............ 5·180
Greatest variation in 24 hours 0·330.—Number of changes 16.
Therm. Max. 71°. May 6. Wind S.E.—Min. 41°. May 13. Wind N.E.
Range 30°.—Mean temp. of exter. air 56°·11. For 31 days with ☉ in ♉ 54·79
Max. var. in 24 hours 24°·00.—Mean temp. of spring-water at 8 A.M. 48·51

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the atmosphere in the evening of the 23rd ... 96°
Greatest dryness of the atmosphere in the afternoons of the 5th and 18th 49
Range of the index 47
Mean at 2 P.M. 59°·0.—Mean at 8 A.M. 64°·7.—Mean at 8 P.M. 72·2
—— of three observations each day at 8, 2, and 8 o'clock .... 65·3
Evaporation for the month 4·80 inches.
Rain in the pluviameter near the ground 1·94 inches.
Prevailing wind, S.W.

Summary of the Weather.

A clear sky, 3½; fine, with various modifications of clouds, 17; an overcast sky without rain, 6; rain, 4½.—Total 31 days.

Clouds.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
26	15	25	1	27	22	14

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
3½	2	4	5	½	9	4½	2½	31

General Observations. —The characteristic of this month to the 20th was dry and pleasant, with much warm sunshine; but the last ten days were showery, accompanied with cold westerly gales. The temperature of the air was often variable, and there were several hoar frosts in the first part of the period. The rains that fell here during the last ten days have been of great service to vegetation and the grass lands, and have restored the verdant appearance of the young wheats; for about the 19th the air became arid and blighty, and the roads exceedingly dusty, so that vegetation, &c. began to droop. There appears now to be a more abundant show of fruits than could have been expected, from the extraordinary severity of the last winter, and the weather has been favourable to their setting firmly on the trees. Great blights, however, may still be seen in many places.

On the 5th and 21st instant lightning occurred in the evenings, which was brought on by crossing winds; and distant thunder was heard most of the day of the 23rd. None of the heavy thunder-storms that are said to have happened in different parts of the country, have passed over this place.

The 11th, 12th, and 13th, were very cold days, with prevailing North, and North-east winds.

The mean temperature of the external air this month is a quarter of a degree higher than the mean of May for many years past.

The atmospheric and meteoric phænomena that have come within our observations this month, are four solar halos, four meteors, lightning on two days and thunder on one, and eight gales of wind, or days on which they have prevailed, namely, one from the North-east, two from the East, three from the South-west, and two from the West.

REMARKS.

London.—May 1—4. Very fine. 5. Slight rain in the morning: very fine. 6, 7. Very fine. 8. Heavy rain at noon: very fine. 9. Cloudy: drizzly. 10. Showery. 11, 12. Clear, with stormy wind. 13. Cloudy. 14–20. Very fine and warm. 21. Sultry, with a heavy thunder storm at midnight. 22. Fine. 23. Fine: overcast, with thunder and rain in the afternoon. 24. Very fine. 25, 26 Stormy, with heavy thunder showers. 27, 28. Showery. 29. Fine. 30. Showery. 31. Fine.

Penzance.—May 1. Clear: showers. 2—4. Clear. 5. Fair: clear. 6. Clear: shower. 7. Rain. 8. Fair: showers. 9. Clear: showers. 10, 11, 12. Fair: a shower. 13. Clear. 14, 15. Fair. 16, 17, 18. Clear: fair. 19. Fair. 20. Clear: fair. 21. Fair: rain. 22. Clear: rain. 23. Fair. 24. Showers. 25. Fair. 26. Showers: fair. 27, 28. Clear. 29. Clear: rain. 30. Clear. 31. Fair.

Boston.—May 1. Cloudy. 2—5. Fine. 6. Rain. 7, 8. Fine. 9. Rain and stormy. 10—12. Cloudy. 13. Cloudy: rain early A.M. 14, 15. Cloudy. 16. Fine. 17. Cloudy. 18. Fine. 19—21. Cloudy. 22. Rain. 23. Cloudy: rain with thunder and lightning P.M. 24. Cloudy: rain early A.M.: heavy rain P.M. 25, 26. Fine. 27. Rain. 28. Fine: rain A.M. 29. Fine: rain P.M. 30. Cloudy: rain P.M. 31. Cloudy.

Meteoro-

Meteorological Observations made by Mr. BOOTH *at the Garden of the Horticultural Society at Chiswick, near London; by Mr.* GIDDY *at Penzance, Dr.* BURNEY *at Gosport, and Mr.* VEALL *at Boston.*

Days of Month, 1830.	Barometer. London. Max.	Barometer. London. Min.	Barometer. Penzance. Max.	Barometer. Penzance. Min.	Barometer. Gosport. Max.	Barometer. Gosport. Min.	Barometer. Boston 8½ A.M.	Thermometer. London. Max.	Thermometer. London. Min.	Thermometer. Penzance. Max.	Thermometer. Penzance. Min.	Thermometer. Gosport. Max.	Thermometer. Gosport. Min.	Thermometer. Bost. 8½ A.M.	Wind. Lond.	Wind. Penz.	Wind. Gosp.	Wind. Bost.	Evap. Gosp.	Rain. Lond.	Rain. Penz.	Rain. Gosp.	Rain. Bost.
May 1	30·033	29·988	29·90	29·80	30·08	30·03	29·36	65	44	57	47	62	47	61	SE.	SW.	SW.	W.	...	...	0·170	...	...
2	30·151	30·141	30·10	30·10	30·18	30·17	29·55	67	36	60	45	60	44	57	NW.	NW.	SW.	W.	...	...	...	...	...
3	30·233	30·192	30·08	30·06	30·24	30·18	29·62	64	35	60	44	60	45	56·5	E.	SE.	SE.	W.	0·50	...	...	...	...
4	30·174	30·107	30·00	29·98	30·16	30·10	29·58	66	41	61	46	62	50	57	E.	SE.	E.	W.	...	...	...	...	...
5	30·098	30·028	29·92	29·86	30·08	30·04	29·50	74	51	64	51	68	56	63	SE.	SE.	E.	calm	...	0·03	...	...	...
6	29·896	29·743	29·80	29·70	29·94	29·80	29·36	81	54	62	52	71	55	57	SE.	SE.	SE.	calm	·70	...	...	...	...
7	29·634	29·559	29·50	29·30	29·70	29·60	29·05	79	45	57	50	67	51	60	SE.	W.	SE.	E.	...	...	·480	...	0·04
○ 8	29·595	29·425	29·40	29·40	29·60	29·48	29·06	67	49	57	50	66	49	60	S.	W.	SE.	calm	...	·45	·050	0·250	...
9	29·344	29·316	29·40	29·40	29·36	29·29	28·86	56	45	54	42	58	46	49	S.	NW.	W.	E.	·35	·18	·105	·020	·37
10	29·666	29·408	29·60	29·40	29·62	29·41	29·02	56	44	49	44	61	44	47	N.	NE.	SE.	E.	...	·10	...	·040	1·32
11	29·790	29·672	29·70	29·70	29·77	29·72	29·30	51	41	53	46	54	43	46·5	NE.	E.	N.	E.	...	...	...	...	·07
12	29·879	29·814	29·80	29·70	29·86	29·83	29·27	62	44	55	43	57	42	48	NW.	var.	N.	N.	·25	...	·040	...	...
13	30·163	30·028	30·00	29·90	30·11	29·99	29·53	53	35	56	44	55	41	46	N.	NE.	NE.	NE.	...	...	...	...	·20
14	30·204	30·150	30·10	30·07	30·22	30·18	29·66	65	45	60	45	62	45	48	SW.	SW.	N.	NE.	...	...	...	...	...
☾ 15	30·277	30·237	30·12	30·10	30·29	30·23	29·66	67	39	59	46	61	44	56	W.	W.	SW.	W.	·50	...	...	...	...
16	30·328	30·309	30·25	30·23	30·37	30·32	29·70	72	49	62	47	65	50	58	SW.	W.	NW.	W.	...	...	...	...	...
17	30·251	30·138	30·20	30·10	30·27	30·19	29·61	78	50	65	50	68	53	58	W.	SW.	W.	W.	...	...	...	...	...
18	30·024	29·940	30·05	30·00	30·11	29·98	29·32	76	48	62	50	66	50	63	W.	SW.	SW.	W.	·70	...	...	...	...
19	29·972	29·888	29·90	29·90	29·98	29·97	29·20	70	52	62	51	67	52	60·5	W.	W.	SW.	W.	...	...	...	...	...
20	29·958	29·917	29·85	29·72	29·96	29·84	29·43	70	50	62	50	66	54	55	SE.	SE.	E.	E.	...	...	...	·010	...
21	29·824	29·743	29·62	29·60	29·80	29·74	29·35	68	50	60	52	69	54	51·5	E.	SE.	E.	E.	·60	·36	·315	·230	...
● 22	29·897	29·745	29·70	29·70	29·96	29·88	29·34	67	49	64	51	66	52	50	SW.	SE.	W.	E.	...	...	·235	...	·36
23	29·922	29·697	29·68	29·64	29·87	29·82	29·40	71	55	63	53	67	56	52	SE.	SE.	SE.	E.	...	·21	...	·345	·32
24	29·731	29·576	29·70	29·68	29·80	29·71	29·17	70	47	64	53	62	52	56	SW.	SW.	NE.	E.	·35	...	·145	...	·36
25	29·594	29·465	29·60	29·50	29·72	29·58	29·08	66	57	62	50	65	52	60	W.	SE.	SW.	W.	...	·64	...	·090	·20
26	29·506	29·442	29·63	29·60	29·61	29·47	28·77	65	49	57	49	61	50	56	W.	NW.	W.	W.	...	·06	·235	·200	...
27	29·688	29·532	29·72	29·70	29·69	29·61	28·92	66	49	57	47	62	49	51	NW.	NW.	NW.	W.	·25	·03	...	·060	·13
28	30·031	29·833	30·00	29·75	30·02	29·88	29·30	64	45	58	48	61	44	53	W.	NW.	N.	E.	...	·05	...	·065	...
☽ 29	30·087	29·926	30·00	29·90	30·13	29·94	29·50	65	50	61	47	63	52	54	SW.	SW.	SW.	W.	...	·20	·640	·540	·05
30	29·841	29·819	29·85	29·80	29·88	29·85	29·15	62	47	58	49	60	50	54·5	SW.	W.	SW.	W.	...	·16	...	·090	·57
31	30·001	29·900	29·95	29·90	30·08	29·97	29·26	66	57	61	50	63	52	56·5	SW.	W.	SW.	SW.	·60	...	...	...	·02
	30·328	29·316	30·25	29·30	30·37	29·29	29·31	81	35	65	42	71	41	54·8					4·80	2·47	2·415	1·940	4·01

THE
PHILOSOPHICAL MAGAZINE
AND
ANNALS OF PHILOSOPHY.

[NEW SERIES.]

AUGUST 1830.

XIV. *A Sketch of the Structure of the Austrian Alps, &c. &c.* By the Rev. A. SEDGWICK, *Woodwardian Professor, Fellow of Trinity College Cambridge, President of the Geological Society, F.R.S. &c.;* and RODERICK I. MURCHISON, *Esq. Sec. G.S. F.R.S. F.L.S. &c.**

[With a Plate.]

§ I. *Introduction.*

WE believe that several years have elapsed since any account of the structure of the Alps has proceeded from the pen of an English geologist. A paper by Professor Buckland, published in the Annals of Philosophy of 1821, threw a new and unexpected light on the geological relations of the whole chain; showing, by many striking details, the analogy between some of its formations and a part of the English series; and proving that the great northern and southern calcareous zones belong exclusively to the secondary period. About the same time Mr. Bakewell was employed in making a series of observations on different parts of the Alps, which led him to similar conclusions †. Of the many excellent memoirs published on the same subject by the geologists of the Continent, we are not now called upon to speak.

The elevation of the Alps during the tertiary period, first asserted by Dr. Boué, has been confirmed, with innumerable details, by the successful labours of MM. de Buch and Elie de Beaumont, and is now generally admitted. A large series of strata on the southern flank of the chain, once regarded as primitive and afterwards elevated to the transition

* Read before the Geological Society, May 21, 1830; and communicated by the Authors.

† See "Travels in the Tarentaise." 2 vols. 1823.

class, has been finally referred to the age of the lias. Numberless strata, formerly described as primitive or transition, have also been successfully compared with the green-sand and the newer secondary formations. These discoveries belong to the recent history of geology*. Many new and careful observations must, however, be made before the great deposits of the Alps can be brought into a rigid comparison with the secondary formations of the North of France, of the North of Germany, and of the British Isles. The peculiar mineral structure of the Alpine limestone—the great mountain masses of it, so modified by crystalline power as to lose the traces of depository origin—the frequent absence of organic remains, and of those well-defined beds of clay and sandstone which form the best subdivisions of the English secondary groups—the enormous faults and dislocations produced during the different epochs of elevation—all these causes have greatly obscured the minuter subdivisions of the different systems of the chain: so that in the Geological Map of Germany (only finished last year, under the superintendence of one of the greatest naturalists of Europe) nearly the whole of the secondary calcareous zones are still represented by one colour, and described as the *Calcaire indéterminé des Alpes*†. Under these circumstances we have ventured to throw into a connected point of view our own observations, made in the summer of 1829 during several traverses among the eastern parts of the chain. We offer them as nothing more than an imperfect sketch, filled up in some instances by materials derived from others: we are anxious to state matters of fact correctly; and we wish that in every instance they should be carefully separated from any theoretical conclusions we may have derived from them.

The following are the principal directions in which we made traverses through the eastern portions of the Alps. 1st, From Vienna to Gratz over the Sömring: 2nd, From Gratz to Laybach, over a portion of the primary axis and the southern calcareous zone: 3rd, From Laybach to Capo d'Istria, over the calcareous chain, which is a prolongation of the Julian Alps, and forms the water-shed between the basin of the Danube and the Adriatic: 4th, A complete traverse by the gorges of the Tagliamento and the crests of the Tauern Alp to the valleys of Salzburg: 5th, Through the lateral valleys of the northern calcareous zone between Salzburg and Inspruck: 6th, Across the calcareous zone by the Seefeld pass to the plains

* We here refer to various memoirs of MM. Elie de Beaumont, Boué, &c. &c.

† Geological Map of Germany, published by S. Schropp and Co., Berlin.

plains of Bavaria. To these may be added, a complete traverse, made by one of the authors of this paper in the summer of 1828, from the subalpine plains of Italy, over the Brenner and Seefeld pass, to the plains of the Danube.

In all these regions the structure of the Alps, when considered only in a general point of view, is of great simplicity; the chain being composed of an axis of primary and transition rocks, chiefly of a slaty texture, flanked and surmounted by the two great secondary calcareous zones; which are in their turn surmounted by the tertiary sandstones and conglomerates, descending on one side into the plains of Italy, and on the other into the elevated plains of the Upper Danube. We have endeavoured to show, in papers read at former meetings of this Society, that the chain of the eastern Alps has been elevated at successive epochs; and (agreeably to the views first published by Dr. Boué) that it has undergone a great movement since the tertiary period. By these successive elevations the relative position of its subordinate parts has been greatly deranged; the northern and southern calcareous zones being in many places completely rent asunder, and having their component strata thrown into the most violent contortions.

To the east of that part of the valley of the Inn which intersects the Alpine limestone, the derangements of the chain are, we believe, in no instance so great as to produce an entire inversion in the order of the calcareous formations; and the transition rocks and red sandstone series on the flanks of the central axis are uniformly surmounted by the lower part of the system of Alpine limestone. But on the west side of the great chasm through which the Inn escapes into the plains at the northern foot of the Alps, the dislocations are more complex; as there appear to be two distinct axes of elevation ranging nearly parallel to each other; one along the true geological central line of the Alps; the other through the centre of the northern calcareous zone. The effect produced by the second axis is such, that some of the higher members of this zone are carried, with an inverted dip, directly against the central chain, and appear to pass under it. This singular derangement of the strata passes through the dolomitic peaks of the Rhetian Alps at Mittenwald; but how far it ranges towards the west, we had no means of ascertaining: we have been informed, however, that similar derangements may be traced into the heart of Switzerland.

Having thus noticed the general position of the great mineral masses of the eastern Alps, it may be expedient briefly to explain the transverse section which forms the chief basis of our observations, and to which nearly all the subdivisions

 of

of the several formations will be referred*. This section commences in the marshes on the shores of the Adriatic, passes through the plains of Udina to the foot of the Alps, intersecting the tertiary marls and conglomerates on the left bank of the Tagliamento: then traverses the calcareous chain (connected with the Julian Alps) to the longitudinal valley between Tarvis and Ponteba, in a part of which is the outcrop of the sandstone, red marl, and conglomerate inferior to the Alpine limestone. It then crosses a low longitudinal ridge of doubtful character, but based upon, and partly composed of transition rocks, and is continued through the secondary, dolomitic ridges of Bleiberg to the valley of the Drave, which here, like so many of the longitudinal valleys of the Alps, forms the boundary between the primary and secondary ridges of the chain. It will be seen by the general section, that the Bleiberg dolomites rest on red sandstone and red gypseous marls; and that the secondary system is based on grauwacké, which contains beds of limestone with fossils resembling those of transition limestone. From the Drave the section is continued through the primary region of the Alps, over the Katsberg, to the valley of the Mur at St. Michael. These primary ridges are chiefly composed of micaceous schists, the beds generally ranging parallel to the principal chain, and dipping nearly due south. From the valley of the Mur the section is prolonged over the crests of the Tauern Alp to the longitudinal valley of the Enns at Radstadt. This part of the line cuts through a great succession of mineral masses, some of which, though eminently crystalline and primary in external character, contain a few subordinate beds of limestone with organic remains. The dip of these transition beds is irregular; but on the northern flank of the Tauern Alp, and in the ridges near Radstadt, the strata become less crystalline, and the prevailing dip is unequivocally towards the north. From Radstadt the section crosses some of the newer transition rocks graduating insensibly into the conglomerates, red sandstone, and gypseous marls; which here, as on the Italian side, form the base of the whole Alpine limestone system. The line is then continued through the great precipices of the older Alpine limestone; through the middle system with the subordinate saliferous marls; and through the younger Alpine limestone, flanked by the sandstones, marls, and conglomerates of the tertiary formations.

The principal section is drawn in such a direction as to show the position of the unconformable beds of Gosau, and the commencement of the tertiary system at the foot of the Traunstein.

* See Plate II. fig. 1.

To

To explain more fully the structure of this part of the Alps, we have added another parallel section (fig. 2), about thirty miles west of the former, from the primary rocks at Gastein across the transition formations of the Upper Salza, the red sandstone series of Werfen, the older Alpine limestone of the Tännen Gebirge, the saliferous deposits of Hallein, the younger Alpine and hippurite-limestone of Untersberg, to the tertiary plains of Bavaria.

After these introductory remarks, we may proceed to notice each of the successive deposits seen on the line of transverse section, and on other corresponding parallels of the eastern Alps. They may be subdivided into the following natural groups in the ascending order. 1. Primary crystalline rocks forming the central axis. 2. Crystalline rocks with calcareous beds containing a few traces of organic remains; the system graduating into rocks agreeing with the ordinary transition type. 3. Red marl, sandstone, and gypsum, &c.; containing in parts of their range large, subordinate masses of magnesian limestone. 4. Older Alpine limestone. 5. Alpine limestone with subordinate saliferous deposits. 6. Younger Alpine limestone. 7. Tertiary formations.

§ II. *Successive Formations of the Eastern Alps.*

1. *Central Axis of primary Rocks.*

The primary rocks of the central axis have their culminating peaks on the eastern borders of the Tyrol, where the Grosse Glockner and the Venediger Alp rise to the respective elevations of 11,775 and 11,698 Vienna feet* above the level of the sea. To the east of these peaks the central ridges diminish gradually in elevation; separating, in their range, Carinthia on the south from the Salzburg country on the north. Following the direction of the Mur for a considerable distance, they are finally divided into two irregular branches; one of which is prolonged in a south-easterly direction into the Bacher Gebirge, and forms the south-western boundary of the tertiary basin of Gratz; the other is continued in the direction of the principal axis of the Alps, and forms the great boundary between the tertiary basins of Vienna and Styria. This latter branch, after disappearing under some of the recent deposits connected with the Vienna basin, emerges near Presburg, and is said finally to die away in the low ridges extending thence towards the north-east†.

It

* 12,281 and 12,201 English feet.

† The gradual diminution in the elevation of the central axis, as it ranges from

It forms no part of our object to give any detailed account of the mineral structure of the central axis; we may however observe, that as it decreases in elevation in its range towards the east, its prevailing character of granitoid gneiss seems to give way to that of mica-schist and some other primary slaty rocks.

In the higher part of the valley of Gastein the principal rocks we observed were the following: (1) Granite; (2) Granitoid gneiss, alternating near the highest waterfall with thin beds of white, granular marble; (3) A very feldspathic variety of mica-schist, here and there containing garnets; (4) The same variety often passing into white-stone, also here and there becoming quartzose and graduating into slaty quartz-rock; (5) Mica and chlorite slates, &c. &c. Some of these varieties below Hof Gastein, and close to the transition series, alternate with thin beds of a beautiful greenish, striated marble resembling *cipollino*.

Above Gasteiner Bad several of the slaty masses contain small disseminated crystals of auriferous pyrites; and in one of the precipices in the highest part of the valley, gold mines have been worked at the height of 7800 Vienna feet above the level of the sea. Similar mines have been worked in the adjoining peaks of the Raurisberg at the height of 9080 feet above the same level *.

In the traverse we made through the primary system over the Katsberg, from Spital on the Drave to St. Michael on the Mur, the prevailing rocks were mica-schist; and we have already remarked that for many miles along the great longitudinal valley of the Drave they have a decided dip to the south, carrying them unequivocally under the transition rocks, hereafter to be noticed, which form the base of the Bleiberg ridge.

Between Gmünd and Rennweg the rocks contain abundance of garnets; and not far from the latter place there are subordinate masses of serpentine in the mica-schist. On the south ascent of the Katsberg, the prevailing rock is a beautiful

from the higher peaks of the Tyrol towards the east, and the great expansion of the southern calcareous zone of the Alpine chain, produce an effect upon the hydrography of the whole region which deserves notice. The calcareous zone not only becomes greatly expanded; but near the commencement of the Julian Alps gives off a second chain, extending into the provinces on the eastern shores of the Adriatic. The consequence is, that the parting of the waters between the Black Sea and the Adriatic takes place, in all the country east of the Adige, among the higher elevations of the secondary calcareous system. West of the Adige, the parting of the waters takes place among the crests of the central axis.

* In English feet the respective heights are 8135 and 9470.

ful chloritic slate; alternating here and there with thin bands of light blue and green, laminated and granular limestone, somewhat resembling the *cipollino* of the Italians; sometimes also alternating with thin quartzose bands, identical with the beautiful green, quartzose, slaty masses which abound in a corresponding part of the pass of the Brenner. With the beds of the Katsberg terminates what we consider the primary system of our transverse section. It is not however possible to draw any precise line of demarcation between this and the superior transition system; for here, as well as in the valley of Gastein (see Pl. II. fig. 1, 2), the two classes of rocks seem, through the intervention of chloritic schist with thin bands of limestone, to pass insensibly into each other. We have thought this fact worth stating, although it is probably a mere local accident of structure, and of no real importance in the history of the successive formations*.

2. *Crystalline Rocks containing calcareous Beds with traces of organic Remains, graduating into Rocks conforming to the ordinary Transition Type, &c. &c.*

The prolongation of the section over the Tauern Alp, from St. Michael on the Mur to Radstadt on the Enns, is through the lower transition system of the Alps. Near St. Michael, beds similar to those of the Katsberg alternate with large masses of crystalline limestone; and for some miles north of that place, the abundance of limestone among the other crystalline rocks begins to give a new character to the country. In our passage through it, we however thought that we were still in the primary system of the chain; and we can hardly express the surprise we experienced when we first discovered, near the village of Tweng, mica-slate with garnets, and chlorite-slate with thin layers of white crystalline limestone, alternating with, and passing into a more thick-bedded limestone, part of which was of a dark blue colour, of less crystalline texture, and contained many encrinital stems.

In ascending the mountain above the last-mentioned village, we found the same dark-coloured encrinite-limestone

* In using the terms *primary* and *transition*, we have only endeavoured to conform to the language current among geologists. The two classes of rocks cannot, perhaps, in any case be precisely separated from each other. It is true that among the stratified rocks of a considerable part of the central axis there are no traces of organic remains: but they may once have existed, and have been obliterated by subsequent crystalline action: and the central peaks of true granite are (at least in their present form) probably among the most recent mineral masses of the chain.

repeatedly

repeatedly alternating with chlorite and mica-schist, also with fine, glossy clay-slate containing crystals of pyrites. Higher up the mountain are great masses of thin-bedded white crystalline limestone overlaid by thickly bedded, blue limestone; and the whole system passes into a series of lofty calcareous peaks, the highest of which, in the Radstadter Tauern, is 9762 Vienna feet above the level of the sea. The highest point of the calcareous peaks belonging to this system, east of the Weiss Eck, is 8360 Vienna* feet above the same level†. We had no opportunity for examining any of these elevated peaks of the chain; but from their mode of weathering we were led to suppose that some of them were of dolomitic structure.

On descending the north flank of the Tauern Alp we found the same blue, thick-bedded limestone alternating with, and graduating into micaceous and chloritic schist, and appearing by a north-easterly dip to be carried under the peaks of the Radstadter Tauern.

When we first found the organic remains in the calcareous rocks above described, we supposed that the whole system was probably an outlier from the great northern calcareous zone, altered in structure by its contact with the primary formations. The sections from the foot of the Tauern to the banks of the Enns, proved that this opinion was untenable. The rocks above described are not only interlaced with the central system of the chain, but are inferior to a long series of strata, considered, if we mistake not, as transition by all the geologists who have described the Alps.

This conclusion is made still more clear by the sections laid bare in the gorge between Hof Gastein and Lend; and on the banks of the Salza between Lend and Werfen‡. The primary rocks of the valley of Gastein are supposed to terminate above the gorge with some great masses of micaceous and chloritic schist containing subordinate, thin beds of crystalline limestone. These masses are succeeded, in the ascending order—(1.) By an enormously thick calcareous series, generally of slaty texture, but obscurely stratified, often arranged in great vertical masses with an irregular cross cleavage: the limestone is here and there so mixed with dark shale as to

* In English feet the respective heights are 10·182 and 8708.

† In the Geological Map of Germany (published by Simon Schropp and Co. of Berlin), all the region of the Alps between St. Michael and Radstadt is erroneously represented as mica-schist. The calcareous chain connected with the Radstadter Tauern extends westwards to Scheideck and Dragstein. The Weiss Eck on the south, and Kalk Spitz on the east, are detached masses belonging to the same system.

‡ See Plate II. fig. 2.

become

become argillaceous and of earthy texture; the strongest beds of limestone are sometimes highly crystalline, generally of a dark blue colour, and contain innumerable white, contemporaneous veins running in irregular lines transverse to the beds. (2.) Micaceous and chloritic slates with a nearly vertical cleavage, having in many places the structure of primary rocks. (3.) Similar slaty masses, but of more incoherent texture, irregularly mixed with calcareous matter, and with calcareous beds generally of a dark blueish colour and of a close subcrystalline texture. This system extends to the bottom of the gorge, and is continued for some way below Lend on the right bank of the Salza. (4.) Fine talcose slate mixed with and passing into serpentine. (5.) A series of beds composed of coarse micaceous slate, sometimes of nearly incoherent texture, mixed irregularly with beds and masses of blueish gray, subcrystalline limestone, which pass in some instances into a fine, white, crystalline dolomite. (5.) Great masses of grauwacké slate, here and there becoming earthy and passing into shale. (6.) Great beds of limestone, some of them white and crystalline, plunging at a high angle into the valley above St. Johann.

We found no traces of organic remains in this long series of deposits, from their commencement above the gorge in the valley of Gastein: but we are convinced, both from their mineral structure and range, that they are prolonged into, and form a part of the system of the Tauern Alp.

On the banks of the Salza, between St. Johann and Werfen, there are some repetitions of mineral structure similar to those above described; but on the whole the beds gradually present a coarser and more mechanical texture: the shales are less micaceous and chloritic, and the slaty masses alternating with them often pass into grauwacké. With this change of structure there is a much greater regularity in the dip; the whole of this upper system being carried, by an undeviating inclination towards the north, under the great precipices of secondary limestone.

A still higher series of rocks on the banks of the Salza are of such a character that it seems uncertain whether they should be referred to the transition or secondary class. They consist of variously coloured shales passing into grauwacké-slate, alternating with greenish gray or reddish fine-grained sandstone; and subordinate to them are some beds of highly calcareous shale and of limestone, in texture resembling some varieties of our transition or mountain limestone. The sparry iron ore of Winterwald, a little south of Werfen, appears to be associated with these ambiguous strata.

We did not examine in detail any of the great deposits of sparry iron ore which characterize the eastern Alps. Although not, perhaps, on the same exact parallel, they chiefly occur under the system of secondary, red, gypseous marl and sandstone, along with a fine grit or grauwacké which is more or less calcareous. According to information which we owe to His Imperial Highness the Archduke John, who from personal examination is most intimately acquainted with the mineral structure of every part of the eastern Alps, the following are among the most important localities where the ore has been extracted for use:—Freisnitz near the Sömring; Rickenau; Neuberg; Veitsch; Niedereibl; Goldrath near Marianzell; Eisenerz; Radmar; Admont; Lietzen near Aussee; and Winterwald south of Werfen*. To the south of the iron ore runs a low chain of limestone more or less argillaceous, which between Neuberg and Admont is said also to contain several small veins of iron spar. In two localities, one near Aussee, and the other at Imlau Graben near Marianzell, the ore is said to overlie red gypseous marls. From all these facts we conclude, that the greatest part of these remarkable deposits are close to the confines of the lowest secondary group of the Alps, and perhaps in some instances enter partially into its composition†.

It would be incompatible with our present object to enter into any longer details respecting the structure of the great series of rocks above described: but it may be asked how we prove them to be of the transition class, since the organic remains imbedded in them exhibit no specific characters. Considering the crystalline structure of some of the secondary formations of the western Alps, it would be difficult to meet this objection, had we not found an answer to it in the facts exhibited by our transverse section on the south side of the central axis.

On the right bank of the Drave below Paternion the formations of crystalline slate are surmounted by grauwacké, shale, and red sandstone, forming the base of the Carinthian lead hills. These formations are not so well exposed, but the order appears to be precisely the same, as on the north side of the axis. A great fault ranges up the valley of Bleiberg, between

* To the same Illustrious Personage we are indebted for many other details connected with the immediate objects of this paper. The section Pl. II. fig. 4, which we owe to Professor Rippl, shows that the geological relations of the great deposits of sparry iron ore near Eisenerz are similar to those above described.

† This conclusion is, we believe, in accordance with the opinion of Dr. Boué.

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the Schlossberg and the Erzberg, producing an upcast to the south-east, in consequence of which the red sandstone and red gypseous marl forming the base of the metalliferous dolomites, are once more brought out, and the whole secondary system is seen to rest unconformably upon inclined beds of grauwacké passing, in some places, into a millstone conglomerate, in others, into a grauwacké slate which is here and there calcareous, and contains subordinate, thin beds of dark-blueish coloured limestone.

In the calcareous grauwacké slate there are many stems of encrinites; and the beds of limestone contain innumerable fossil shells chiefly of the three genera Producta, Spirifer, and Terebratula. Hand specimens of these rocks could not be distinguished, either by their structure or by their fossils, from English mountain or transition limestone: and it deserves remark, that in one of the sections below the village of Bleiberg, thin calcareous bands full of transition fossils mark the dip and bedding of the rock; while its slaty cleavage (precisely as in some of the calcareous transition slates in the north of England) is transverse to the stratification.

The fossil shells collected by ourselves are:

Producta hemisphærica;
Producta latissima;
Producta, a species resembling P. Martini;
Pecten, resembling a species in the transition series of Cork.

And to this list may be added,

Encrinites, fragments of Spirifers, &c. &c.

These facts appear to decide the question respecting the age of the system of beds we are describing, as far as regards the country south of the central axis: and to place the corresponding beds on the north side of the axis in a different system, would be a violation of the plainest rules of analogy. It seems therefore to follow, that certain important inferences, recently drawn from the non-existence of transition rocks with organic remains in some portions of the western Alps, have no application to the eastern parts of the chain here described*.

* By reference to M. Rengger's memoir in the *Denkschriften der allgemeinen Schweizerischen Gesellschaft* (Zürich 1829), it will be seen that transition rocks form an equally important band in a part of the western Alps, and that on their southern or inferior limit they graduate downwards from grauwacké slate into mica-slate and gneiss; whilst in an ascending series they pass through iron ore deposits into red sandstone and conglomerate. So that in the western division of the chain, as well as in the eastern, they separate the Alpine or Jura limestone (synonymous terms) from the primary rocks.

3. *Red and variegated Sandstone and gypseous Marl; sometimes with subordinate Beds of fetid Limestone, Rauchwacké, &c. &c.*

This deposit is, we believe, found nearly throughout the whole extent of the eastern Alps overlying the transition series, and forming the base of the great precipices of the older Alpine limestone. Its characters are so peculiar, and its continuity is so remarkable, that, independently of its importance as a term of comparison with other regions, it would well deserve a separate notice.—On the northern side of the transition system of the Alps, the red sandstone is exposed in a series of longitudinal valleys, and overlies nearly all the principal deposits of sparry iron ore, of which we have already pointed out the general range.

From Werfen on the Salza to Häring on the Inn, it ranges in the position we have indicated, forming a succession of red terraces; and, in consequence of a great increase in the thickness of the subordinate fetid limestone, spreads out into a succession of low ridges of hills ranging parallel to the escarpment of the Alpine limestone. On the west side of the Inn it is no longer visible, in consequence of the great dislocation pointed out above, which brings the higher beds of the calcareous zone into contact with the central axis.

On the south side of the central axis the deposit has the same position, and we believe also the same continuity; and it is well exposed in the valley between Ponteba and Tarvis; also on both sides of the Bleiberg hills, as is indicated in our transverse section.—In order to convey some notion of the structure of this formation, we proceed to notice one or two sections on both sides of the chain, where it is well exposed.

In describing the succession of deposits on the banks of the Salza, we have already pointed out certain beds of close-grained grauwacké sandstone and variegated marls, which seemed to form a passage between the true transition system and the red sandstone. About half a mile south of Werfen are some quarries in the secondary series, exposing the following succession of beds in the ascending order:

(1.) Irregular masses of red conglomerate with rounded pebbles of the older rocks. (2.) Black, calcareous shale passing into dark smoke-gray, fetid limestone, the whole irregularly traversed by veins of gypsum. (3.) Argillaceous beds striped with parallel, red bands of gypsum. (4.) A thick, irregular cellular bed with much hydrate of iron, thin veins of gypsum forming reticulations through the mass. (5.) A mass composed of several beds of gypsum, finely granular, but not fibrous,

fibrous, and at its upper surface alternating with gray gypseous marls. (6.) Thin beds of dark fetid limestone surmounted by beds of gypsum lost under the alluvium of the hill. The aggregate thickness of these beds is fifty or sixty feet, and their dip is north. Near the same place is another quarry exposing red and variegated gypseous marls, alternating with beds of red sandstone perfectly like the new red sandstone of England.

On the whole, the formation we are describing is ill exposed, the greater part of it being obscured by the accumulations of alluvial matter near the foot of the great calcareous precipices; and we have only pointed out the previous sections for the purpose of conveying some notion, however imperfect, of its internal structure. Its thickness is very considerable, but its lower limit is very ill defined. Its upper portion seems to pass into the Alpine limestone through the intervention of dark calcareous shales with subordinate beds of fetid limestone.

On the right bank of the Drave, south-east of Paternion, the red sandstone is much concealed, but the order is as follows: (1.) Grauwacké; (2.) Red sandstone; (3.) Shale and fetid limestone; (4.) Dolomite of the Erzberg.

The great fault which brings up the red sandstone series under the Schlossberg, on the left bank of the rivulet below the village of Bleiberg in Carinthia, exposes the following succession of phenomena*: (1.) Contorted and broken beds of shale and bands of compact limestone. Some of the limestone is fetid; many of the beds of shale are meagre and micaceous, of a gray and blueish gray colour; others are red or variegated, more argillaceous, and contain irregular and earthy, nodular masses of gypsum. This system is traversed by one of the adits to the lead-works, which intersects many beds of stinkstone like those under the dolomites on the north side of the Erzberg.

(2.) Red sandstone alternating with red and variegated gypseous marls, not to be distinguished from the most characteristic beds of new red sandstone. This system rises from beneath the contorted shales, and in its prolongation abuts against some highly inclined beds to be next enumerated.

(3.) Highly inclined beds composed of coarse grauwacké, passing here and there into a conglomerate with rounded pebbles of quartz and primary rocks. The cementing principle is close-grained and micaceous, and so hard that the masses are quarried for millstones.

(4.) A second mass of red marl and sandstone overlying and

* See Plate II. fig. 3.

abutting

abutting against the preceding group; also in its prolongation abutting against highly inclined beds of grauwacké slate.

(5.) A series of highly inclined beds of grauwacké slate, succeeded or cut off by an irregular mass of trap.

(6.) Trap, which traverses the ravine and occupies the banks of the rivulet for several hundred feet. It is close-grained, of a dark greenish colour, and breaks into irregular fragments at a number of earthy ferruginous joints. We have no specimens of this rock; but it may we think be regarded as a variety of the black porphyry, or *melaphyre* of De Buch. It contains several fragments of white crystalline limestone, and here and there passes into a conglomerate with a cement of iron clay.

(7.) The trap is immediately succeeded by grauwacké slate, with the subordinate thin beds of limestone above described, containing Encrinites, Spirifers, Productæ, &c. Further down the valley, and at the south base of the Schlossberg, the red sandstone series is seen for several miles overlying the grauwacké series and underlying the Alpine limestone.

The great faults, which have not only brought out the red marl and sandstone, but have made the dolomite of the Erzberg to abut against the grauwacké of the Windisherberg*—the singular contortions and breaks of the formations—the appearance of the trap and trappean conglomerates, not merely the accompaniments but probably the causes of the disruptions—the unusual occurrence of organic remains decidedly of the transition class—the clear relations (notwithstanding the local confusion) of the transition and secondary systems—all these circumstances together make the sections in the neighbourhood of Bleiberg the most instructive we have seen in the Alps.

On the south side of the chain, porphyry is said to be in many instances subordinate to the red sandstone. In the valley above Ponteba we found red sandstone passing into conglomerate with pebbles of porphyry; and in the range of the formation, towards the west, porphyry becomes so extremely abundant as to predominate over, and in some instances *almost to take the place of*, the red sandstone†.

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* Plate II. fig. 1.

† The range and extent of this porphyry is well marked in M. Ployer's Map of the Tyrol. One of the sections which best exhibits its relations and intimate connection with the gypseous red sandstone, was well seen between Neumarkt and Cavalese, in a traverse, from the valley of the Adige to Predazzo and the Val di Fassa, made by one of the Authors of this paper in 1828. (Pl. II. fig. 7.) The lowest beds observable in ascending from Egna or Neumarkt, on the Adige, to the pass of St. Lugano leading to the Val d'Avisio, and thence to Predazzo, consist of red sandstone with greenish marls

It remains for us to notice the sections on the right bank of the Inn, through the red sandstone series; especially as we have there a good exhibition of that expanded portion of it which contains great subordinate beds of limestone. We have in a former paper noticed the masses of red marl, red sandstone, and conglomerate, which on the right bank of the Inn rise from beneath the Alpine limestone and form the support of the unconformable tertiary deposits of Häring. The sandstone passes, near the coal-works, into a very close-grained, thick-bedded, micaceous rock; and alternates with masses of limestone resembling rauchwacké, some of which are bituminous and extremely fetid. From this place to Schwatz (a distance of more than two posts), there are, on the right bank of the Inn, a great succession of sections showing alternations of fetid limestone and red marl or sandstone*. South-

marls and reddish gray calciferous grits. By following these on their rise towards the north-east, they are found to repose upon a sandstone, which passes downwards into a recomposed porphyry, and finally into a coarse conglomerate. This conglomerate at Lugano is seen to rest against the mountain masses of crystalline porphyry, from which it has been derived, and to which it appears to have precisely the same relations as the older conglomerates of Foyers, Trefad, and the [Ord of Caithness, have to the granitic and syenitic rocks of the Highlands. (See Geol. Trans. 2d Series, vol. iii. p. 139.)

In an ascending order, these conglomerates, graduating into sandstones and grits, exhibit in one part a fine-grained whitish grit, like some of the best freestone of the Scotch coal-measures; and these beds contain impressions and detached portions of plants, and thin laminæ of carbonaceous matter. The red marls and red sandstone which succeed, immediately support the Alpine limestone of Monte Cislone; and the highest members of that mountain assume the prevailing dolomitic aspect and shivery structure of all the peaks of Alpine limestone in this part of the Tyrol. The same relations of red sandstone, marls, and grits, supporting the Alpine limestone, are seen well exposed in the adjoining valleys of Avisio and Fassa, wherever the general range and structure of these formations is not obscured by the trap dykes abounding in that region, many of which have a syenitic and granitic character. In the higher part of these valleys, and particularly near Predazzo, these dykes frequently burst through the regular strata, which in many instances are altered in their mineral character, dislocated, and frequently overwhelmed by bulging masses of the igneous and intrusive rocks. At the Canzocoli, the Alpine limestone in contact with the trap, is changed into a crystalline, white marble, which is now quarried for statuary purposes; the edges of the limestone presenting bands of serpentine, and the whole forming a beautiful and direct analogy to the altered lias (described by Dr. Macculloch) in the Isle of Skye, where that deposit comes into contact with syenite.

The red sandstone and marl which have been derived from the pre-existing porphyry, and which so uniformly support the Alpine limestone, contain subordinate protuberances of anhydrite and gypsum near Cavalese; and the rock-salt, recently discovered in the adjoining valley, is probably subordinate to the same deposit.

* The transverse section from Häring to Schwatz makes a very acute angle

South-west of Schwatz this system terminates in red conglomerates, and is seen distinctly to rise up to, and repose upon, the older formations.

In this part of the range the limestone greatly predominates in the exposed sections; for the red marls and sandstones being more destructible, do not form precipices, but occupy small longitudinal valleys. The contortions of some of the limestone beds, and the nature of their alternations with the sandstone, are well exposed in sections south of Rattersberg. We believe that the greatest number of these limestone beds are magnesian, some of them are metalliferous, and nearly all of them are fetid*. Their structure is extremely various. Some of them are compact, some white and crystalline, some yellow and earthy, and some cavernous. When struck with the hammer, some of the masses shiver into innumerable fragments; and one of these varieties, of a beautiful white colour, has externally the small columnar structure of dried starch, and when struck falls into minute grains with trapezoidal faces. The name of *rogenstein* has been given to this variety, which however presents no analogies to the *rogenstein* of the Hartz.

This limestone series has often been called transition. We have allowed the difficulty of drawing a line between the secondary and transition systems of the Alps; and in this instance we derive no assistance from organic remains, for a few casts of Terebratulæ are the only fossils we saw in the beds we are describing. They are, however, too much interlaced with the lower part of the red sandstone to be separated from it; we agree therefore with Dr. Buckland in considering them secondary: and if the red sandstone of the Alps be identified with the new red sandstone, they approach both in position and in mineral character, much nearer to the *zechstein* or magnesian limestone than to any other formation with which we are acquainted. At the same time we wish carefully to distinguish them from the great zone of older Alpine limestone, from which they are separated by the red sandstone and gypseous marls, and to which, according to our views, they are in no respect subordinate.

4. *Older Alpine Limestone.*

By the term Alpine limestone we mean the limestone found in any part of the great secondary calcareous zones superior

angle with the range of the red sandstone system, and therefore conveys an erroneous idea of its thickness; which, however, after every deduction, must be very considerable.

* Most of the old works below Schwatz were, we believe, in veins of argentiferous galena. They are now deserted.

to

to the red sandstone. The calcareous beds described in the preceding section are not therefore included under this term; which has undoubtedly led to some mistakes, having been applied to the *zechstein*, to the oolitic series, and to true transition rocks. It is not well that the same name should designate formations so widely separated from each other; but limiting the term Alpine limestone as we have done, we think that it may be used with advantage, and that it can lead to no confusion. This great deposit admits, as we have already stated, of three natural subdivisions, which we proceed to notice in order*. It is, however, entirely foreign to our purpose, to give many details respecting a formation so well known, and so often described by those who have possessed incomparably better means than we had of studying the details of its mineral structure: we shall therefore in a great measure confine ourselves to a brief notice of some of the phenomena on our lines of section.

The first division, the older Alpine limestone, appears on our transverse section in the dolomites of the Bleiberg and the great escarpments of the Tannen Gebirge. We have before noticed the marls and thin beds of fetid limestone below the village of Bleiberg, which seem to form a passage into the superior limestone series. The thin-bedded, fetid limestone also appears in great force near the northern base of the Erzberg, overlying the red sandstone, and passing into the superior dolomites.

Near Werfen the order observed at the base of the calcareous zone was as follows:

1. Thin, dark, bituminous beds with calcareous veins; some of them fetid, and the masses shivering into angular fragments.
2. Light-gray limestone, breaking into similar fragments.
3. Dark, fetid beds, alternating with dark, micaceous shale throwing out copious springs of water.
4. Beds of limestone of compact texture, much traversed by calcareous veins, and used for marble. The beds separated into vertical masses by many great perpendicular clefts.
5. Gray limestone and shale.
6. Dark beds of limestone, traversed by contemporaneous veins, alternating with dark shale, into which they pass in-

* The subdivisions of the Alpine limestone here adopted, were we believe first given by M. de Lill, of Hallein, a gentleman whose investigations have thrown great light on the natural history of the secondary formations of the chain. The Geologists of the French school seem disposed to reject the term Alpine limestone altogether, and to substitute in its place the term Jura limestone. We are unwilling to exclude the term Alpine limestone from what we think its proper place, and are only anxious to give it a consistent meaning, which may lead to no mistakes.

 sensibly,

sensibly, and become of earthy texture.—This system is surmounted by the great, gray, bare precipices of lower Alpine limestone many thousand feet in thickness. It appears, therefore, that in the structure of the lowest beds of the Alpine limestone there is, to say the least of it, a very strong analogy between certain parts of the great northern and southern zones.

Dark, bituminous, slaty beds are not, however, confined to the base of the series, but in some instances, for example in the Seefeld pass, compose whole mountain masses. In consequence of the very complex dislocations of that part of the calcareous zone, it is difficult to ascertain the exact place of the bituminous ichthyolites of Seefeld. But placing the dolomites of Mittenwald as the mineralogical centre, we think that the Seefeld schist is considerably inferior to the saliferous deposits of Hall, and that some of the beds of the system descend far down into the lower Alpine limestone.

It would be useless for us, after all that has been written on the subject, to attempt any general description of the dolomites of the Alps. The metalliferous hills of Bleiberg are chiefly composed of this variety of rock, arranged in great irregular beds dipping south at an angle of 70°. The lead ore of these hills is arranged in masses which appeared to us parallel to the beds of limestone: but our persevering and skilful young friends, Messrs. J. and R. Taylor (who had previously visited the mines and examined them with great care) have since convinced us that the ore is deposited in true veins; which, though they for considerable spaces range parallel to the strata, cut obliquely through them at certain intervals. In the dolomitic beds of the hill above the village, are a few casts of shells and other obscure fossils; and we were happy to recognize among them two or three specimens of the Gryphæa incurva. Immediately behind the village are some beds of shale and limestone (fire-marble), with many fossils; among which are the celebrated iridescent Ammonites, which Dr. Buckland, we believe, identified with the fossils of the lias. Considering, then, that in this single locality we have grauwacké with beds of limestone containing transition fossils, surmounted by red sandstone and gypseous marls; and this latter series again surmounted by fetid limestone, dolomites, &c. containing the Gryphæa incurva, and by other beds containing Ammonites and Belemnites—there can, we think, be no doubt to what part of the secondary series we should refer the lowest division of the lower Alpine limestone as it is developed in the Bleiberg Hills.

Notwithstanding the magnificent scale of the sections in the

Salzburg

Salzburg country, the geological evidence on that side of the chain is not so complete as in the neighbourhood of Bleiberg. Ammonites and Belemnites have been found, though rarely, in the lower part of the great precipices which surmount the red sandstone series near Werfen; but we are not aware that the Gryphæa incurva has ever been seen along with these fossils. The argument from analogy is, however, sufficiently convincing where we have nothing to oppose to it. We therefore venture to conclude, that the great system of the older Alpine limestone, overlying the red sandstone, commences on both sides of the chain with the lias. Its precise superior limit we are not able to indicate; but we believe, from its enormous thickness, as well as from some of its fossils, that it ascends considerably into the oolitic series*.

5. *Limestone with subordinate saliferous Marls, &c. &c.*

No mines of salt have, we believe, been worked in the formation of red sandstone and gypseous marls which underlies the older Alpine limestone. All the principal deposits of that mineral are in a much higher position; being incased in the calcareous system of the Alps, and distinctly overlying the older Alpine limestone. This fact appears to be incontestably proved in the memoirs of M. de Lill de Lilienbach†; to whose kind assistance and instructions we were greatly indebted during our visit to the Salzburg Alps.

* Along the line of our transverse section, we in vain looked for that structure which is so characteristic of many of the secondary rocks of England; and in no instance did we discover a single specimen of true oolite. In the range of the calcareous zone along the southern flanks of the Bacher Gebirge, and from thence towards the lower regions bordering on the Danube, the rocks, however, occasionally exhibit the external characters of a part of our oolitic series. Thus, near Gonowitz, we found beds of a yellow colour, and a coarse open texture, somewhat resembling cornbrash, and containing many well preserved fossils. At the time, we were not aware of the rarity, and consequent interest, of these phenomena; which are well deserving of a much more careful examination than we bestowed upon them.

On the great road from Cilly to Laybach, there are some highly interesting sections, especially near Fränz and St. Oswald, where great beds of shale alternate with coarse, reddish sandstone resembling millstone grit; the whole being surmounted by masses of rauchwacké and other modifications of dolomite. In the same neighbourhood are several beds of coal, which we wished to visit, as we supposed them analogous to the coal deposits we had seen, subordinate to the newer secondary system, south-west of Vienna. We were however disappointed in our hopes of seeing them; and we have been since informed that they all belong to formations of tertiary brown coal. We mention these facts, not in the expectation of throwing any light upon the structure of this portion of the Alps, but in the hope of directing the attention of future observers to the interesting phenomena of the calcareous chain between Gonowitz and Laybach.

† *Zeitschr fur Mineralog.* No. x. 1828; and *Bulletin des Sciences, Mai* 1829, &c. &c.

In several parts of the chain the older Alpine limestone is succeeded by a great series of beds, composed of limestone, calcareous gritstone, sandstone, and shale. The calcareous beds are often nearly compact, sometimes cherty, commonly separated by thin bands of marl, and offer many varieties of colour and structure. The other members of the system are liable to like variations. The beds of marl, shale, and sandstone sometimes become greatly expanded; and the whole system frequently becomes contorted to such a degree, that great masses of brecciated sandstone, shale, &c., are rolled up and enclosed in mountain masses of the limestone we are now describing. To what causes the phenomenon may be due we do not inquire; but it is, we believe, among such brecciated and contorted masses that all the rock-salt of the eastern Alps has been discovered. The several deposits, commencing at Hall, and ranging through Berchtolsgaden, Hallein, Halstadt, Ischel, and Aussee, are certainly not now, and probably never were, continuous; but they have all been formed under nearly similar circumstances, and are all nearly on the same parallel. We proceed briefly to notice one or two sections where the true position of the rock-salt is indicated; and for fuller details we must refer to the papers of M. de Lill and other writers on the structure of the eastern Alps.

(1.) *Salt deposit of Hall.* (Pl. II. fig. 5.)—The accompanying section (for which we are indebted to M. Pöhringer, *Ober Bergmeister* at Hall) shows the true position of the rock-salt among the calcareous mountains on the left bank of the Inn. The whole system has a regular dip of about 30° towards the S.W., and belongs to that part of the secondary chain which, in consequence of an enormous dislocation, seems to plunge under the central axis (see above p. 83). The lowest beds of limestone in the section rise into the peaks of the Lavatscherberg, in which are found many iridescent Ammonites. They are succeeded by some compact, red beds resembling those immediately below the salt in some other localities. These red beds are surmounted by bands of anhydrite, forming the base of a great saliferous mass, composed of green, red, gray, and variegated marls, brecciated masses of sandstone, fetid limestone, &c., in which all appearances of stratification are entirely lost*. The whole thickness of this mass, measured in the direction of the section, is about 1250 English feet; and

* This position of the anhydrite seems to be nearly similar to that which has been noticed by Charpentier, Bakewell, and other writers on the gypseous and saliferous masses in the Alps.

galleries

galleries have been opened in it at the height of 4480* Vienna feet above the level of the sea: but, like all the other corresponding deposits, it appears soon to thin out, so as not to be continued through the neighbouring mountains. The brecciated saliferous mass is surmounted by three beds of the united thickness of fifty feet. The lowest (*c*) is chiefly composed of marl; the middle bed (*b*) of gypsum; and the highest (*a*) consists chiefly of a gray and white concretionary limestone, the nuclei of the concretions being often composed of small Terebratulæ. To this rock the term *rogenstein* has been applied, as well as to the singular concretionary magnesian rock near Schwatz (*supra*, p. 96); but they both differ essentially, in their structure and relations, from the *rogenstein* of the Hartz, which is a coarse oolite subordinate to the new red sandstone. Over these beds come the upper strata of limestone containing many fossils; among which we recognized Ammonites, Belemnites, Buccinites, Pectens, Terebratulæ, &c. &c.

The whole of the beds here described, both from their position in the chain and from their fossils, belong evidently to the middle system of Alpine limestone; and notwithstanding the brecciated structure of the saliferous mass, they seem to be less disturbed than in the other analogous deposits of the Alps.

(2.) *Salt formations of Berchtolsgaden and Hallein.*—These two deposits, though very near together, are not continuous; probably in consequence of the great dislocations and contortions of the neighbouring portions of the chain. The relations of the salt mass of Hallein are extremely clear and may be seen even in our very reduced section (Pl. II. fig. 2). It is in a lenticular form, being about 1520 toises in length, 600 in breadth, and 220 † toises in its greatest thickness; and it is imbedded, or rather enclosed, in the contorted strata of limestone. The limestone below the salt is thin-bedded and compact; in colour gray, red, or variegated; alternates with bands of green and red marls, and contains here and there considerable portions of chert. It is surmounted by dark-coloured, gypseous marls which form the lining of the salt deposit ‡. The saliferous masses are made up of green, red, and variegated gypseous clays, much mixed with brecciated masses of red micaceous sandstone, and with fragments and concretionary masses of dark-coloured limestone. The whole system is surmounted by twisted beds of light-gray, compact limestone, some of which contain many fossils. These relations are proved, not only by the

* 4672 English feet. † 9722, 3837, and 1407 English feet.

‡ In the galleries through the dark coloured gypseous marls there is an extraordinary efflorescence of sulphate of magnesia.

general

general structure of the whole region, but also by numberless internal traverses through every part of the saliferous mass. It is therefore obvious, that the salt of Hallein is subordinate to strata separated from the lower red sandstone and gypseous marls, above described (p. 92), by many thousand feet of the older Alpine limestone*.

3. *Saliferous mass at Ischel.*—The salt deposits of Halstadt, Ischel and Aussee are, we believe, very nearly on one geological parallel; and in their structure and relations to the nearest portions of the chain, they seem to be almost identical. The accompanying plan of the Ischel works †, which we owe to the kindness of M. Dicklberger the *Ober Bergmeister*, will convey a correct notion of the position of the salt mass among the beds of limestone. The neighbouring hills to which the salt is subordinate, in consequence of a great flexure, dip to the south-west. The beds under the salt are argillaceous, and contain some bands of dark-coloured limestone with casts of Ammonites and some bivalves. Over these beds, and immediately under the salt mass, are some thin, compact, cherty beds of limestone. The salt mass is a confused, irregular compound of gypseous and saliferous marls, &c.; which has been worked, at the lowest level, through a breadth of about 500 Vienna feet, and through a depth, between the highest and lowest levels, of about 1500 feet. These different levels are approached by means of 12 horizontal galleries cut through the inferior beds, as represented in the plan. It deserves remark, that here, as at Hallein, the saliferous mass (E) is separated from the surrounding limestone by bands of dark-coloured gypseous marls (D) not saliferous. The superior beds of limestone (F) are hardly to be distinguished, either by their structure or their fossils, from those which underlie the salt.

These details are sufficient to explain the nature and position of the great saliferous deposits of the eastern Alps, which evidently occupy an intermediate position between the older and younger portions of the calcareous zones; and may therefore, along with the accompanying strata, be conveniently regarded as one of the natural subdivisions of the Alpine limestone, in which so many of the distinctive characters of secondary formations are almost entirely wanting.

* The relations of the saliferous beds of Berchtolsgaden are not so obvious; but we have little doubt that they are nearly in the same geological position: for the system is continued northwards across the valley, and passes finally under the ridge which is a prolongation of Untersberg. On this account we think that the saliferous beds of Berchtolsgaden cannot, by any complex series of faults, be brought into comparison with the marls inferior to the older Alpine limestone.

† Plate II. Fig. 6.

If

If it be asked in what part of the secondary series we place the salt deposits above described, we are unable to give any very definite answer to the question. In the limestone beds associated with the salt are many fossils; among them are Ammonites, of which the concamerations are marked by simple or undulating lines, and Orthoceratites. Both these fossils might be supposed to indicate strata older than any part of the oolitic series. Along with them are however near Hallein, oval Ammonites, and spheroidal masses resembling organic remains of the green-sand; also several casts of shells resembling oolite fossils, and a singular body, found in our Kimmeridge clay, to which the name Tellinites solenoides has sometimes been given. At Aussee, in the beds of limestone containing the saliferous marls, there are, along with other fossils, corallines of the genera Tubipora and Astræa, and Pentacrinites. On the whole we are disposed to place the salt formations of the Alps high in the oolitic series.

The preceding conclusion might appear strange to one who had only studied English geology; but it cannot now be considered anomalous, as recent discoveries have established the existence of salt among rocks of almost all ages. In the Crimea it is said to be daily accumulating in inland lakes. In Poland it probably exists among tertiary deposits. In the Austrian Alps we have placed it among the upper oolites. In Switzerland Mr. Bakewell places it in the lias. In Wirtemberg Alberti has proved it to be both in the *Keuper* and *Muschel-kalk*. In England, though all the great salt mines are in the new red sandstone, there are two or three copious salt springs in the coal-measures. Lastly, in certain parts of the United States, salt springs are stated by Mr. Featherstonehaugh to issue from old transition slate rocks*.

6. *Younger Alpine Limestone.*

Under this designation we include all those portions of the northern secondary chain which are superior to the saliferous deposits of the Alps. As we were unable to define the upper limits of these deposits, we are necessarily unable to define the lower limit of the formation we are now attempting briefly to describe. We, however, suppose that it commences somewhere in the middle or upper system of the oolitic series; and it terminates on the outskirts of the chain, in ridges of indurated shale, sandstone, and limestone; in some places containing many characteristic fossils, and now supposed, by most of the geologists who have visited the region, to be the equivalents of the green-sand and chalk.

* See Phil. Mag. and Annals, N.S. vol. vii. p. 200.—Edit.

Our

Our examination of the calcareous zone on the south flank of the Alps was much too hasty to enable us to establish the three subdivisions we are now attempting to illustrate. We believe however that they may be traced, and that the zone may in many places be distinctly divided into older Alpine limestone and younger Alpine limestone, separated from each other by a system of strata composed of thin-bedded limestone, alternating with shales, gypseous marls, &c. One or two sections, kindly shown to us by Professor Rippl, exhibited this succession: and if these gypseous marls be considered as the representatives (in a somewhat altered form) of the saliferous system of the northern Alps, the subdivisions of the two great calcareous zones will be in perfect accordance.

In the southern Italian Alps, the younger formation of limestone has been examined with great care by MM. Maraschini and Catullo; and the latter gentleman has, by the help of a great suite of organic remains, proved the existence of beds of the age of the green-sand, overlying rocks containing many organic remains of the oolitic series. Near Belluno, Feltri, Canal di Brenta, &c., the system terminates in a red and white fissile limestone (*scaglia*) containing many flints; which, from its structure, position, and fossils, has been indentified with the chalk.

Between Adelsberg and Trieste the limestone beds contain many fossils, and among them are innumerable Nummulites. How far these fossils descend in the secondary series, we are not able to determine. In the ascending order, the formations, before they reach the Adriatic, undergo a great change in external character. The calcareous beds (chiefly composed of a compact, light-gray limestone full of Nummulites) no longer predominate; but become subordinate to great masses of blueish gray micaceous shale, and of sandstone generally of a gray or greenish gray colour, and here and there containing a few traces of carbonaceous matter. Along the shores of the Adriatic, for several leagues south of Trieste, the micaceous shale is so abundant as to produce a succession of ruinous cliffs, apparently held together only by the subordinate bands of sandstone and nummulite-limestone. We believe that this system is now generally regarded as the representative of the green-sand or chalk—a conclusion which is in perfect accordance with our views of the structure of the district.

It is not our intention, in such a sketch as this, to attempt any detailed description of the younger secondary formations of the Austro-Bavarian Alps; we must, however, notice some of their varieties of structure, and some of the masses which are subordinate to them. Occasionally they pass into dolomites,

mites; in which case they generally rise into peaks, weather into peculiarly fantastic forms, and lose all traces of stratification. From this it would seem, that their mineral structure has originated in some great crystalline action, which commenced after the deposition of the calcareous mass. We must however observe, that the same external forms, the same crystalline texture, and the same obliteration of all traces of depository origin, may be found in numberless parts of the chain where the rocks contain no magnesian earth.

Gypsum is found, in several places, subordinate to the younger Alpine limestone: for example, at Faulenbach near Fussen, where it is extensively quarried. The same mineral is found, near the head of the Kochel See, high in the series, and close to the tertiary plains of Bavaria. It is there associated with black, blue, and red fetid marls, and with fetid, porous rauchwacké, not to be distinguished in external character from the magnesian limestone of England. We mention this to show the hopelessness of attempting to determine the age of the different portions of Alpine limestone by mere mineral structure. We may further remark, that gypsum is found in the Alps among secondary rocks of all ages, and is therefore, by itself, no test of the age of any of them. We have already shown, that it exists in the marls inferior to the older Alpine limestone, in the superior saliferous marls, and also among beds high in the series of the younger Alpine limestone: and in former communications we have shown that the same mineral is also found, in considerable abundance, among the tertiary formations of Salzburg and Bavaria*.

In the extreme prolongation of the calcareous zone into the ridges which terminate the chain a few miles south-west of Vienna, coal-works have been opened, in one or two places, under the direction of Professor Rippl. The coal is of bad quality, and is subordinate to shale alternating with the Alpine limestone. We obtained no fossils from the neighbourhood of the works which we visited; but we suppose, from their position in the chain, that they are in the higher part of the system we are describing.

One of the most abundant rocks of this series is a peculiar, light-gray, compact limestone, well known to every one who has visited the Alps; but entirely unlike any secondary rock

* On the north-western bank of the Walcher See, near the side of the great road from Inspruck to Munich, are traces of some works which were opened about twenty years since in search of quicksilver. We were informed that some traces of that metal had been found among the argillaceous masses which there alternate with the beds of younger Alpine limestone.

of England. It is well exhibited in some of the beds of Untersberg; which on the north-west flank of that mountain contain innumerable Hippurites of the same species with those found, both in Provence and the Pyrenees, among rocks supposed to be of the age of the green-sand *.

The Hippurite beds, in their prolongation towards the east, pass under a great series of white and red indurated marls, containing, we believe, some chalk fossils. These are surmounted by gypseous marls, with bands of calcareous grit containing Nummulites; which are in turn surmounted by a great system of beds, composed of sandstone (*molasse*), conglomerate, shale, and marl; some of the highest of which (in ravines below Schweiger Mill, &c. &c.) contain fossils similar to those in the overlying beds of Gosau. (See Pl. II. fig. 1 & 2.)

We think these highest beds, from their position in the section as well as from their fossils, are superior to the chalk; and on that account we in a former paper called them tertiary. We at the same time stated that we regarded them as a term of an undefined series, to be interpolated between the *calcaire grossier* and the chalk, and that we did not pretend to draw any well-defined line between them and the secondary system.

We have already mentioned the ridges of hills, composed of shale, sandstone, and limestone, developed on the outskirts of the Salzburg and Bavarian Alps close to the tertiary system. Being more thin-bedded and of less firm texture than the older parts of the chain, they have been exposed to extraordinary breaks and contortions; sometimes dipping towards the mineralogical axis, and sometimes from it; in one place being vertical, in others twisted into saddle-shaped masses; and, if we mistake not, being in some instances absolutely inverted. This series appears to be greatly expanded near the eastern termination of the Alps, and has been described by Dr. Boué, with many excellent details, under the name of Vienna sandstone (*grés Viennois*). In the maps and memoirs of M. Keferstein, it is designated by the name of *Flysch*.

From the outskirts of the calcareous zone near Reichenhall to the valley of the Rhine, it forms, in the position we have pointed out, a nearly continuous succession of ridges, easily distinguished from the inner portions of the chain. In some parts of the system the beds of limestone almost disappear, and it then passes into a formation of sandstone and shale, hardly to be separated, without the help of fossils, from the superior

* We have the authority of Mr. Lyell, for stating that near Cape Passero in Sicily, Hippurites occur in a tertiary formation newer than the Sub-apennine: but most frequently they seem to occur in the newer secondary formations.

tertiary

tertiary groups. In other places the calcareous beds of these outer ridges are of much greater thickness, and they then exhibit all the usual mineralogical characters of Alpine limestone.

The Kachel-stein (immediately south of the iron works of Kressenberg, near Teisendorf) exhibits the following characteristic succession. (1.) Light-gray, calcareous marls with indurated bands resembling *planer-kalk*. (2.) Blueish, micaceous marls with calcareous bands, some of which are composed of calc-grit; others of compact, argillaceous limestone resembling blue and white lias. (3.) A great series of blueish gray flag-stones, alternating with marls, generally blue, but here and there of red or greenish red colours. (4.) Micaceous sandstone, mostly thin-bedded, and of a greenish gray colour, but containing some thick beds extensively quarried for building. Some of the calcareous bands of this lofty ridge contain Ammonites, and Belemnites; and it appears to be separated by a double system of faults, on one side from the metalliferous mountains of Alpine limestone, and on the other from the newer ferriferous strata of the Kressenberg*.

In the Alpen Spitz immediately south of Nesselwang, beds of compact limestone, with many Belemnites, occasionally passing into a more earthy texture (like indurated *planer-kalk*) and containing balls of pyrites, alternate with bands of calc-grit and thick beds of dark-coloured shale.

In some instances the rocks of this system exhibit the ordinary characters of the green-sand of England. Thus at Sonthofen the iron-sand is not distinguishable in mineral character from the lower green-sand of the Kentish denudation; and it contains many fossils, among which we may enumerate Ammonites, Belemnites, Inocerami(?), Nummulites, Pectens, Terebratulæ, numerous crustacea, &c. &c. We had no hesitation in considering this deposit as subordinate to the higher part of the younger Alpine limestone, and therefore secondary†.

At Haslach, a few miles south of Bregenz, there is a deposit of red oxide of iron in beds of calcareous shale, alternating with beds of calc-grit and limestone, some of which are of a bright green colour. The series seems to pass under great precipices of Alpine limestone much charged with Nummulites. At Oberdorf, in the immediate neighbourhood, is a

* We have the authority of the *Berg-Meister*, who has many years superintended the iron works, for asserting (in confirmation of the published statements of Count Munster), that the Kressenberg beds contain no Ammonites or Belemnites. In our opinion the formations of the Kachel-stein and Kressenberg, though in close contact, ought not to be confounded.

† Dr. Boué has erroneously stated, in different journals, that we had considered the Sonthofen deposit as tertiary.

 nummulite-

nummulite-limestone, apparently subordinate to great beds of indurated marl resembling *planer-kalk*. The geological relations at the last-mentioned places are rather obscure; but the deposits are we think unquestionably secondary, and nearly on the same parallel with those at Sonthofen.

On the whole we concluded, that the different portions of the ridges above described, which appear on the outskirts of the Alps, were nearly of the same age with the green-sand and chalk formations of England. The conclusion seems to be borne out by the position of the subordinate beds, as well as by their fossils, and is in some instances also confirmed by their mineral contents.

It was our wish in this sketch of the structure of the Eastern Alps, to avoid mere matters of detail, as being incompatible with our object. But the short abstracts of some of our former communications having been misunderstood, and consequently misrepresented; it became necessary, in this account of the younger Alpine limestone, to explain our views, more at length than we first intended, by referring to two or three specific localities.

7. *Tertiary Deposits**.

Having in two papers, read last year before the Geological Society, explained our views respecting the tertiary deposits of the Austrian Alps; we should not have added anything to what we have already stated, had it not been necessary to notice certain comments on these communications which have been published by Dr. Boué †.

1. He is mistaken in supposing that we confounded the iron sand of Sonthofen with rocks of the tertiary age. We distinctly stated that it contained Ammonites and Belemnites, and alternated with beds of the younger Alpine limestone; from which we concluded that it was secondary; and this conclusion is given, though very shortly, in the published abstract of our paper (Phil. Mag. and Annals, N.S. vol. vii. p. 53). On the age of the Sonthofen beds there appears therefore to be no difference of opinion between ourselves and Dr. Boué: but he has been led to misrepresent our meaning, from knowing nothing of our communications except through the medium of abstracts, which from their brevity may be easily misunderstood.

* By tertiary deposits we mean all regular beds, of whatever age, newer than the chalk.

† See New Edinburgh Philosophical Journal, Jan. 1830, p. 176; *Bulletin des Sciences, Fevrier* 1830, p. 228—*Juin* 1829, p. 328; Abstract of the Proceedings of the Geological Society (Phil. Mag. and Annals of Philosophy, for the last month, p. 64–67), &c.

2. He

2. He states that the tertiary formation of Häring is entirely of freshwater origin. We prove that it contains several species of marine shells; from which we conclude (contrary to the opinion of Dr. Boué, but on evidence we think not short of demonstration,)—that the marine tertiary deposits of the Alps do sometimes ascend far up the transverse secondary valleys.

3. He contends that the tertiary formations on the flanks of the Austrian Alps commence with the superior divisions of that class of rocks, the lower divisions being entirely wanting. We, on the contrary, have shown, both by transverse sections and suites of fossils, that some of the inferior groups of the tertiary deposits in the Gratz basin are of the age of the London clay. So far there is a difference between Dr. Boué and ourselves on what we consider questions of fact.

4. We also differ from him considerably on questions of opinion. For example: he describes great masses of calcareous conglomerate in the Salzburg Alps, which he compares with a part of the *nagelflüh* of Switzerland, and places in the secondary system under the green-sand. We have not examined the *nagelflüh*, and can therefore offer no opinion respecting its age; but of late years it has been generally considered tertiary. In the Salzburg Alps, the great masses of calcareous conglomerate are chiefly found on the outskirts of the chain, and form the base of a new series of deposits which are physically and zoologically separated from the older system; and are, if we mistake not, newer than the chalk. Occasionally, as in the valley of Gosau, they appear far within the chain: but in that case they are unconformable to the rocks which surround them; and there is then no means of determining their age, except by the help of their fossils, or by comparing them with the corresponding beds on the outskirts of the chain. After making use of both these means of comparison, we concluded that the overlying conglomerates of Gosau were newer than the chalk: and in our examination of other parts of the eastern Alps we did not find any large masses of coarse, calcareous conglomerate subordinate to the newer secondary system.

5. M. Boué seems to attribute much greater importance, than we do, to mere mineralogical distinctions. We know numberless instances in which green-sand above the chalk cannot be distinguished from green-sand below the chalk. Some of the lacustrine formations of central France have been mistaken for old secondary deposits. We have found, on the outskirts of the eastern Alps, fossils of the London clay alternating with rocks resembling our coal grits, and masses of conglomerate like those subordinate to the oldest secondary rocks; and

and we have shown, that the youngest tertiary shells of Lower Styria are sometimes associated with a beautiful oolite, undistinguishable in hand specimens from the great oolite of Bath. On these accounts we think that mineral character alone is nearly useless in comparing the ages of tertiary formations widely separated from each other.

6. He appears to identify the two deposits of iron ore at Sonthofen and Kressenberg. They both occur in a ferruginous green-sand with many Nummulites, and their mineralogical resemblance is nearly complete. But Nummulites by themselves prove nothing; and it may be asked whether all the circumstances of these two deposits are such as to justify this identification. We think not. For the Sonthofen iron ores contain Ammonites and Belemnites, and (if we rightly understood the plans of the works) are interlaced with the secondary system of the Alps. On the contrary, the Kressenberg deposit contains no Ammonites or Belemnites, and is entirely on the outskirts of the secondary system; so that its age can only be made out by its internal structure and its fossils. Now there is nothing in the mere structure of the Kressenberg beds to prove that they are secondary; and an elaborate examination of their fossils by Count Munster gave the following results.

(1st.) Of 172 species of these fossils, 42 exist in, and are characteristic of, the tertiary formations of Germany, England, France, and Italy.

(2nd.) There are 3 species, 2 of which resemble, and one of which (Ostrea semiplana) is identical with, certain fossils of the chalk.

(3rd.) Of the remaining 126 species, some are new and others indeterminable; but for the most part they belong to such genera as are commonly found in tertiary formations.

(4th.) Of the characteristic chalk-fossils, (viz. Ammonites, Belemnites, Hamites, Scaphites, Turrilites, &c. &c.,) there is not the least trace. Neither are there any traces of the Gryphæa columba, of Inocerami, plicated Terebratulæ, &c. &c.

(5th.) The only fossils (excepting the three above mentioned) which at the first glance seem to belong to the chalk, are a Plagiostoma and a Gryphæa. But on a closer examination, they not only differ from fossils of the chalk, but are of the same species with certain fossils found in the tertiary formations of Ortenburg and Sternberg.

Such are the statements of Count Munster. And no reply has been, or can be, given to them; unless it can be shown that the fossil species have been erroneously determined. But this has not been attempted. We therefore adopt the conclusion

clusion of Count Munster, that the iron-sand of Kressenberg is a formation newer than the chalk*.

With the same spirit of generalization, of which we have been speaking, formations widely separated from each other, in the Alpine and Carpathian chains, have been brought under comparison; sometimes by the help of mineralogical characters, almost unassisted by a single organic fossil. Nor do we complain of this where no better evidence is to be had. On the contrary, we owe the greatest obligations to MM. de Lill, Boué, and other writers who have thrown much light on the structure of parts of Europe which have been seldom visited. At the same time, in all questions of doubt, we must take care not to allow ourselves to be misled by mere words; and in settling any difference of opinion, we must never apply to one formation the properties of another in a distant region, because it passes under the same name. For example; in arguing respecting the age of the overlying beds of Gosau, we have no right to transport the reader over 150 miles of Alpine limestone, and then to assert, that (at Grünbach, Piesting, &c.) the *same deposit*, as that at Gosau, contains Belemnites and certain other secondary fossils. In the present state of our information, and on questions of doubt, such an argument is nothing better than a direct inversion of the rules of induction.

7. After the preceding remarks, we are prepared to enter on the question of the age of the overlying deposit of Gosau. Let it be borne in mind—that it is identical with formations at the base and outskirts of the chain, and that it is equally difficult to account for its present position among the serrated Alpine peaks, whether it be considered secondary or tertiary—that the chain has undergone great movements of elevation within the tertiary period—that the older divisions of the tertiary groups do exist in certain portions of the eastern Alps—

* M. Boué appears to assert that Ammonites and Belemnites are found in the Kressenberg deposit (*Bulletin des Sciences, Juin* 1829, p. 329.). On the authority of the *Berg-Meister*, who has spent many years in excavating this deposit, as well as from our examination of the spot, we doubt the correctness of this assertion; and it would be to no purpose to tell us that these fossils are found at Sonthofen. M. Boué also states, generally, that the fossils of the green-sand make a near approach to those of tertiary formations: and that some fossils of the oldest secondary rocks at Hall, Bleiberg, and Maibel in Carinthia, have also a tertiary appearance (*Bulletin des Sciences*; and abstract of the proceedings of the Geological Society, July 1830). We do not wish to oppose all parts of this statement; but we think that at least the Bleiberg fossils offer no support to it.—If such suites of fossils, as those described by Count Munster, really occur in the green-sand below the chalk, there is an end of any zoological distinction between secondary and tertiary formations. † See our last Number, p. 65.—Edit.

that

that we have, consequently, no right to exclude them, at least hypothetically, from the Salzburg valleys; and lastly, that the tertiary deposits do sometimes ascend (e. g. at Häring) far up the transverse valleys of the neighbouring portions of the chain. If all this be admitted, we must allow—that there is no great *à priori* improbability, much less any impossibility, that the Gosau beds should be of some age newer than the chalk. If it be further considered—that there is a great break between the *calcaire grossier* and the chalk, which has not yet been filled up—that in the neighbourhood of Maestricht, beds have been found superior to the chalk, and containing a mixture of secondary and tertiary shells*—and that the portion of the chain we are describing, underwent its last elevation since the commencement of the tertiary period; we must then also conclude—that the regions bordering on the eastern Alps are the very places where we ought to look for the presence of ancient tertiary formations.

If the eastern Alps have been elevated at so recent an epoch, may there not have been on their flanks a continuous succession of deposits, between the newer secondary and the older tertiary periods? And is it not further probable, that the older tertiary rocks, having been deposited in deep water, may contain a mixture of pelagian shells not commonly found among the fossils of more shallow basins? All this is undoubtedly nothing but hypothesis: but it has reference to existing facts, and tends to bring the perplexing phenomena of the Alps under those laws by which the development of successive formations appears to have been generally governed.

After all, the age of the Gosau beds must be determined by their relations, structure, and fossils. There is nothing in their relations and structure which, in our opinion, proves them to be older than the chalk: and on examining the Gosau fossils on the spot, it is impossible to deny, that from their state of preservation, the great preponderance of univalves over bivalves, and the incredible abundance of shells of certain genera, seldom found except in the newest formations, the whole group has a decidedly tertiary appearance. At the same time, there are a few shells (Hippurites, Gryphites, Plicatulæ, &c. &c.†) which forcibly reminded us of the fossils of the newer secondary strata.

Since our collection of Gosau fossils reached England, it

* See the abstract of a paper by Dr. Fitton, Phil. Mag. and Annals of Philosophy, Feb. 1830, p. 140.

† The Hippurite of Gosau, is not of the same species with that found in the secondary rock of Untersberg; and we have before remarked that Hippurites are not confined to secondary formations.

has

has been carefully re-examined; and we are enabled, through the kind assistance of our friend Mr. J. de C. Sowerby, to give the following results.

Out of more than one hundred different species (collected by ourselves on the spot) there are from thirty to forty bivalves; and of those capable of being identified, nearly equal numbers are referrible to the youngest secondary, and the oldest tertiary formations*. The univalves are much more numerous, especially in the quantity of each species; a fact seldom remarked in secondary deposits. Among upwards of fifty species, three only are found in the chalk or green-sand, whilst seven species are identical with known tertiary fossils; and several of the genera, such as Volvaria, Pleurotoma, and Voluta, have, we believe, seldom if ever been found in any deposit below the surface of the chalk†.

After all these facts and observations, we venture to reaffirm those conclusions which in previous memoirs we have endeavoured to establish.

1. That in association with different parts of the eastern Alps, is a fine succession of tertiary deposits, commencing with some of the oldest and ending with some of the newest which have hitherto been described.

2. That the older tertiary deposits of the Austrian Alps sometimes ascend far within the transverse valleys, and in such cases rest unconformably upon the beds of secondary limestone.

3. That the same deposits are developed, in many places, on the outskirts of the chain; and do in such cases pass under, and graduate into, those newer deposits to which alone M. Boué would restrict the name of tertiary.

We now return to the southern extremity of our transverse section (Pl. II. fig. 1.) Although there is a general accordance in the successive zones of secondary rock on each side of the central axis, it will be seen, even on the minute scale of our section, that the tertiary deposits of the Friuli form only inconsi-

* The secondary species are: Mya plicata; Corbula elegans; Trigonia scabra; T. alæformis; Pecten quinquecostatus; Exogyra lævigata; E. conica; Terebratula dimidiata; Cucullæa carinata. The tertiary species are Sanguinolaria Hollowaysii; Cytherea lævigata; Cardium hippopæum; Pectunculus nummarius; P. auritus; P. pulvinatus; P. brevirostris; Nucula similis.

† The three univalves, identical with species in the chalk or green-sand, are: Auricula incrassata; Cirrus granulatus; Rostellaria calcarata. The seven tertiary species are: Pleurotoma prisca; Fusus bulbiformis; F. intortus; Rostellaria macroptera; Mitra pyramidella; Voluta citharella; V. coronata (?).

 derable

derable hills, very unlike the elevated masses of the same age in Salzburg and Bavaria. Not far from the gorge of the Tagliamento we found tertiary molasse, conglomerate, and marl, dipping from the nearest precipices of Alpine limestone towards the south; but we could not discover the exact representatives of those tertiary groups described, by one of the Authors of this paper, as occupying the neighbouring district between the Brenta and Piave*. Indeed these groups seem to thin off gradually towards the east: and we lose all vestiges of them beyond the great delta formed by the rivers Piave, Tagliamento, and Isonzo†. In the neighbourhood of Trieste, all the mountains are composed of the younger secondary strata, which in many places come down to the coast and form bold promontories, standing out into the deep sea of the Adriatic.

Description of Plate II.

Fig. 1. Transverse section of the eastern Alps, from the alluvial and tertiary plains of the Friuli on the south, to the valley of the Traun and the tertiary plains of Salzburg on the north.

Fig. 2. Transverse section (parallel to Fig. 1.) from the primary mountains of Gastein to the tertiary plains of Bavaria.

Fig. 3. Red sandstone, grauwacké, transition limestone, &c., as seen on the banks of the rivulet below Bleiberg in Carinthia.

Fig. 4. Position of the spathose iron ore N.W. of Leoben; from a section by Prof. Rippl.

Fig. 5. Section of the salt deposit of Hall near Inspruck.

Fig. 6. Sectional plan of the salt-works of Ischel.

Fig. 7. Section, showing the relations of the Alpine limestone, red sandstone and porphyry, between Neumarkt and Cavalese in the southern Tyrol.

XV. *On the Shortest Distance between two Points on the Earth's Surface.* *By* James Ivory, *Esq. M.A. F.R.S. &c.*‡

IT may not be improper to illustrate the series in the Magazine for last month by applying it to an example; and I shall take the one given by M. Puissant, at p. 42. of the Additions to the *Conn. des Tems* for 1832.

* See Phil. Mag. and Annals, N.S. vol. v. p. 401.—Edit.

† We had no opportunity of studying the interesting phænomena of this delta; but some notion may be formed, even by our small section (Pl. II. fig. 1.), of its rapid increase during the last 1400 years

‡ Communicated by the Author

The

The latitudes λ and λ' of the two stations, and their difference of longitude ψ, are as follows:

	Centesimal.	Sexagesimal.
λ	$= 48^\circ\cdot68315$	$= 43^\circ 48' 53''\cdot41$
λ'	$= 48\ \cdot08414^*$	$= 43\ 16\ 32\ \cdot61$
ψ	$= 0\ \cdot703349$	$= 0\ 37\ 55\ \cdot85$

In a triangle on the surface of the sphere formed by circles between the pole and two points having the same latitudes, and the same difference of longitude as the two stations on the spheroid, the two sides s and s' meeting at the pole are the complements of λ and λ'; the contained angle is ψ; and the third side is the arc σ of the series. The two angles μ and μ', adjacent to s and s', will be obtained by Napier's Analogies; viz.

$$\mu = 139^\circ 22' 51''\cdot85$$
$$\mu' = 40\ 10\ 58\ \cdot17.$$

The inclination i of the side σ to the pole is the complement of the perpendicular drawn to σ from the pole: therefore,

$$\cos i = \sin s \sin \mu = \sin s' \sin \mu', \quad \log \cos i = 9\cdot6718827$$
$$\log \sin i = 9\cdot9458575.$$

$$\text{Further, } \sin a = \frac{\sin \lambda}{\sin i}, \quad a = 51^\circ 39' \ 5''\cdot62$$

$$\sin a' = \frac{\sin \lambda'}{\sin i}, \quad a' = 50\ 56\ 37\ \cdot06$$

$$\sigma = 0\ 42\ 28\ \cdot56$$

According to M. Puissant, r being the radius of the equator, and $r\sqrt{1-e^2}$ the semi-polar axis, we have,

$$\log r = 6\cdot8046154, \quad \log e^2 = 7\cdot8108714.$$

If now we neglect the terms of the series multiplied by e^4, we shall have,

$$\frac{s}{r} = \sigma \sin 1''\left(1 - \frac{e^2 \sin^2 i}{4}\right) - \frac{3e^2 \sin^2 i}{4} \sin \sigma \cos (a + a');$$

and hence,

$$s = 78693\cdot23 + 64\cdot97 = 78658^{\text{m}}\cdot20.$$

By a calculation in which the terms multiplied by e^4 are taken into account, M. Puissant finds $78658^{\text{m}}\cdot1$ for the same distance.

With respect to the azimuths there is a remark to be made. It is usual to consider two stations as two points of the geodetic curve infinitely near one another, and their azimuths as the angles which the curve makes with the meridians. But the azimuths which are really observed and used in practice are independent of the geodetic curve; the azimuth at one

* Both the latitudes are misprinted in the *Conn. des Tems*, p. 42.

station being the angle between the meridian and the vertical circle passing through the other station. Understanding the term in this sense, I shall put m and m' for the azimuths reciprocally observed at the stations of which λ and λ' are the latitudes; and it is obvious that m and m' will coincide with μ and μ' when $e^2 = 0$. In this Journal for September 1828, I have found these two equations, viz.

$$\Delta = \sqrt{1 - e^2 \sin^2 \lambda}, \quad \Delta' = \sqrt{1 - e^2 \sin^2 \lambda'}, \quad Q = \frac{\sin \lambda}{\Delta} - \frac{\sin \lambda'}{\Delta'};$$

$$\frac{\sin \psi}{\tan m} + \cos \psi \sin \lambda - \cos \lambda \tan \lambda' = \frac{\cos \lambda}{\cos \lambda'} \cdot e^2 \Delta' Q,$$

$$\frac{\sin \psi}{\tan m'} + \cos \psi \sin \lambda' - \cos \lambda' \tan \lambda = - \frac{\cos \lambda'}{\cos \lambda} \cdot e^2 \Delta Q:$$

And if we suppose $e^2 = 0$, these equations will be changed into those which follow:

$$\frac{\sin \psi}{\tan \mu} + \cos \psi \sin \lambda - \cos \lambda \tan \lambda' = 0.$$

$$\frac{\sin \psi}{\tan \mu'} + \cos \psi \sin \lambda' - \cos \lambda' \tan \lambda = 0:$$

we therefore have,

$$\frac{1}{\tan m} - \frac{1}{\tan \mu} = \frac{\cos \lambda}{\cos \lambda' \sin \psi} \times e^2 \Delta' Q,$$

$$\frac{1}{\tan m'} - \frac{1}{\tan \mu'} = \frac{\cos \lambda'}{\cos \lambda \sin \psi} \times - e^2 \Delta Q.$$

Now $\cos \lambda' \sin \psi = \sin \sigma \sin \mu$, and $\cos \lambda \sin \psi = \sin \sigma \sin \mu'$; and if we put,

$$x = \cos \frac{\lambda + \lambda'}{2} \cdot \sin \frac{\lambda - \lambda'}{2}$$

$$y = \sin \frac{\lambda + \lambda'}{2} \cos \frac{\lambda - \lambda'}{2}$$

$$R = 0{\cdot}43429 \text{ \&c.}$$

we shall easily obtain the following formulas; viz.

$$\log \sin (\mu - m) = \log \left(\cos \lambda \sin m \times \frac{2 e^2 x}{\sin \sigma} \right) + e^2 y \sin \lambda \, R,$$

$$\log \sin (m - \mu') = \log \left(\cos \lambda' \sin m' \times \frac{2 e^2 x}{\sin \sigma} \right) + e^2 y \sin \lambda' \, R,$$

which serve to compute μ and μ' when m and m' are given. The same formulas will likewise serve to compute m and m' when μ and μ' are given: namely, by first substituting $\sin \mu$ and $\sin \mu'$ for $\sin m$ and $\sin m'$ in order to find near values of m and m'; and these being used in a second operation will bring out the required quantities with all the accuracy that can be desired.

Applying the formulas to M. Puissant's example, I have found,

$$m = 139^\circ 17' \; 4''{\cdot}07,$$
$$m' = \;\; 40 \;\; 16 \;\; 45 \; {\cdot}95.$$

Of

Of these quantities m coincides exactly with the value of the same angle found by M. Puissant (arc z', p. 45); but owing to an error of calculation in the *Conn. des Tems*, there is more than a minute of difference between m' and the angle V'' at p. 46.

The inspection of the preceding formulas is sufficient to prove that whatever be the situation of the stations, sin $(m-\mu)$ may be reckoned equal to sin $(m'-\mu')$; and consequently, $m+m'$ on the spheroid to $\mu+\mu'$ on the sphere: the difference arising from the variation of latitude is of the order e^4, and is always insensible.

If we put M and M′ for the angles which the geodetic curve makes with the meridians of the two stations, the true azimuths m and m' will be indistinguishable from M and M′ so long as the two stations are so near one another that the curve between them may be reckoned in one vertical plane. As the distance between the stations increases, M and M′ separate themselves from m and m', and M+M′ is no longer equal to $m+m'$ nor to $\mu+\mu'$. It may be added that m and m' are exactly equal to μ and μ' when the two stations are equally distant from the equator: but in the like circumstances, M and M′ are not strictly speaking equal to μ and μ', the difference, although extremely small in most cases, increasing as the distance of the stations increases. Precision of ideas seems to require that in such investigations a distinction should be made between the true azimuths m and m', and the angle M and M′.

July 13, 1830. J. IVORY.

XVI. *On a new general Principle of Mechanics. By Professor* GAUSS *of Göttingen**.

IT is well known that the principle of virtual velocities converts the whole science of statics into a mathematical problem; and by D'Alembert's principle the theory of dynamics is reduced to that of statics. It is therefore evident that there can be no new fundamental principle of the theory of motion and equilibrium but what is contained in those two others, and may be deduced from them. This is, however, no reason why every other new principle should be of no value. It will always be interesting and useful to take new views of the laws of nature; some problems may thereby be more easily solved, or a particular fitness may be discovered in them. The great geometrician, who has raised the structure of mechanical science on the principle of virtual velocities in such

* Translated from the original German, in Crell's Journal of Mathematics.

a brilliant

a brilliant manner, has not deemed it useless to give more definiteness and generality to Maupertuis's principle of least effect; and indeed this principle may sometimes be applied with great advantage*.

The peculiar characteristic of the principle of virtual velocities is, that it contains a general formula for solving all problems of statics, and is thus the representative of all other principles of the kind; but its title to the honour of representation is not so clear as to be self-evident by the mere enunciation of it. In this respect the principle which I am going to propose appears to me to deserve the preference; it has besides a second advantage, viz. that of embracing in a perfectly equal manner the law of motion as well as of rest. It is perfectly natural that in the gradual advancement of science, and in the acquirement of it, the easy parts should come in before the more difficult ones, the simple before the more complicated, the particular before the more general; but, at the same time, the mind, once arrived at the higher station, endeavours to reverse this order, and thus views the theory of statics as a particular case only of mechanics. Even the above-mentioned geometrician recommends the principle of least effect, on account of its embracing the theories of equilibrium and of motion at the same time, if the former be so expressed that the living forces should be a minimum in either case. This remark is however more a play upon the word than truth, as the minimum takes place in both cases in very different respects.

The new principle is as follows:

The motion of a system of material points connected together in any manner whatsoever, whose motions are modified by any external restraints whatsoever, proceeds in every instance in the greatest possible accordance with free motion, or under the least possible constraint; the measure of the constraint which the whole system suffers in every particle of time being considered equal to the sum of the products of the

* I beg however to remark, that the manner in which another great geometrician has endeavoured to prove Huyghens's law of the extraordinary refraction of light in crystals having double refraction, by means of the law of least effect, does not appear satisfactory to me. The admissibility of this principle is indeed essentially dependent on the preservation of living forces, according to which the velocities of the moving material points are determined by their places only, without being influenced by the direction of the motion, as is assumed in the theory above alluded to. It appears to me that on the supposition of emanation of light, all attempts to connect the phænomena of double refraction with the general laws of dynamics must prove fruitless, so long as the particles of light are considered as points.

square

square of the deviation of every point from its free motion into its mass. Let m, m', m'', &c. be the masses of the points; a, a', a'', &c. their places at the time t; b, b', b'', &c. the places which they would occupy if entirely free in their motion after the infinitely small particle of time dt, in consequence of the forces acting upon them during this time, and of the velocities and directions acquired by them at the time t. Their real places c, c', c'', &c. will then be those for which of all places compatible with the conditions of the system the quantity $m(bc)^2 + m'(b'c')^2 + m''(b''c'')^2$ &c. is a minimum. The equilibrium is evidently a particular case only of the general law, and the condition for this case is, that $m(ab)^2 + m'(a'b')^2 + m''(a''b'')^2$ &c. itself is a minimum, or that the continuance of the system in a state of rest more accords with the free motion of the single points than any possible change which the system could undergo. Our principle is easily deduced from the two others in the following manner.

The force acting on the material point m is evidently composed, first, of one which in conjunction with the velocity and direction existing at the time t, will carry it during the time dt from a to c; and of a second one, which would carry it in the same time from a state of rest in c through cb, considering the point as free. The same applies to all other points. According to D'Alembert's principle, therefore, the points m, m', m'', must, by the conditions of the system, be *in equilibrio* in the points c, c', c'', &c. when acted upon by no other forces but the second ones, tending to cb, $c'b'$, $c''b''$, &c. According to the principle of virtual velocities the equilibrium requires that the sum of the products of every three factors, viz. of each of the masses m, m', m'', &c. of the lines cb, $c'b'$, $c''b''$, &c. and any motions, compatible with the conditions of the system, projected on those latter lines respectively, should always be $= 0$, as this principle is commonly enunciated*, or more correctly, that that sum should never be positive. If, therefore, γ, γ', γ'', &c. are places, compatible with the conditions of the system, different from c, c', c''; and ϑ, ϑ', ϑ'', &c.,

* The usual enunciation of the principle tacitly supposes such conditions, that the motion contrary to every possible motion should likewise be possible, as for example, that a point must *necessarily* remain on a certain surface, that the distance of two points from one another should be always the same, &c. But this is an unnecessary restriction not always accordant with nature. The surface of an impenetrable body does not force a material point on it, always to remain on it, but only prevents its deviating from it on *one* side; a stretched inelastic but flexible thread between two points renders impossible an increase of distance, but by no means a diminution of it, and so on. Why not therefore at once express the law of virtual velocities so as to embrace *all* cases?

denote

denote the angles which are formed between $c\gamma$, $c'\gamma'$, $c''\gamma''$, &c. and cb, $c'b'$, $c''b''$, &c. respectively, $\Sigma m . cb . c\gamma . \cos\vartheta$ is either $= 0$ or negative. Now we have $\gamma b^2 = cb^2 + c\gamma^2 - 2 . cb . c\gamma \cos\vartheta$; and it is clear that

$$\Sigma m . \gamma b^2 - \Sigma m . cb^2 = \Sigma m . c\gamma^2 - 2\Sigma . m . cb . c\gamma . \cos\vartheta$$

and is consequently always positive, and hence that $\Sigma m . \gamma b^2$ is always greater than $\Sigma m . cb^2$, or that $\Sigma m . cb^2$ will be a minimum. Q. E. D.

It is very remarkable that the free motions of a system, when not consistent with the necessary conditions of the same, are modified by nature in the same manner as the calculating mathematician, following the method of minimum squares, balances results which refer to magnitudes connected with each other by a necessary dependence. This analogy might be further followed up, but this does not lie within the scope of my present design.

XVII. *On the Measurement (by Trigonometry) of the Heights of the principal Hills of Swaledale, Yorkshire.* By John Nixon, *Esq.*

[Continued from page 12.]

IN the winter preceding the survey of Swaledale, some experiments, undertaken to determine on a novel plan the cylindrical error of the horizon-sector, led to the rejection of the original object-glass of its telescope for one of shorter focus, but much superior in the centering. The first trial of its merits took place at Great Whernside on a perfectly calm and remarkably clear afternoon in May; the test being the consistency of repeated observations of the respective depressions of the nearly level summit of Ingleborough and the rounded one of Shunnor Fell; two different, yet equally difficult subjects for the correct pointing of the horizontal wire of a telescope of a moderate power. On examining the angles (corrected for the deviations of the bubbles from their reversing points,) it was remarked that the depression of Shunnor Fell had diminished as the decidedly frosty evening advanced, the decrement amounting to twenty seconds, whilst the nearly contemporaneous depressions of Ingleborough might be considered, one measurement excepted, to have been constant. Now as the Swaledale observations, compared with those of the preceding surveys, present an unusually limited range in the corresponding errors of collimation, the superiority of the present object-glass over the former one may be concluded to be satisfactorily established, and the horary diminution

minution of the depression of Shunnor Fell must be accounted for, notwithstanding the unvarying value of that of Ingleborough, by admitting the existence of local refraction. On a calm day, when the temperature reaches its maximum about noon and declines rapidly towards sunset, the thin stratum of air based on an extensive *plateau*, has a tendency to become rarer about midday than the one immediately above it, but acquires in return a proportionate augmentation of density at nightfall;—the refraction, during the interval, passing in consequence from its least to its greatest value. This has evidently been the case in the observation of Shunnor Fell, where the ray, under circumstances of weather peculiarly disposed to develop local refraction, literally grazed for more than a mile the broad and level summit of Great Whernside. In the direction of Ingleborough, the ground, on the contrary, absolutely precipitous at first, forms a steep declivity to the very base of the mountain. In confirmation of the explanation proposed, it may be added, that the height of Shunnor Fell, calculated from the *mean* of the observed depressions, and with the usual estimate for refraction, comes out within half a foot of the truth. The measurements at Ingleborough, it is proper to mention, were not made by the horizontal wire, but by a new and infinitely superior method hereafter to be described.

The large levels of the sector had preserved their adjustments so nearly unaltered during the survey of Wensleydale, that it was deemed advisable on the present occasion to ascertain their reversing points from time to time, leisurely and under favourable circumstances in the valley; rather than, as had hitherto been the plan, from hurried and generally unrepeated experiments with the instrument resting on such supports (sometimes of questionable stability) as the summit of the fell presented. The satisfactory determination of the two points of the scale between which the bubble of the level will come to rest, in two opposite directions of the telescope within its Ys, is a task requiring extreme precaution and address. The support, based on solid ground, must be perfectly firm; its surface sufficiently ample not to require any part of the instrument to project over it; and without being smooth, which is highly objectionable, it should be selected so nearly a true plane that its contact with the under surface of the stand may extend throughout its length. It is also of importance that the surface should not yield, from the friable texture of the material, to increased pressure. At Reeth, the operation was undertaken with great prospect of success on a pile of several tons of pig lead, but the results, probably from the softness

ness of the metal, were too discordant to be worthy of confidence. When a proper support has been procured, still it will frequently occur that a careful observation has been ruined on reversing the instrument, by disturbing the stand;—by lowering the telescope so rapidly or incautiously as to bring it forcibly in contact with the Ys;—or by a spontaneous deviation of coincidence in the zeros of the arc and index. From the subjoined statement of the position of the reversing points of the two levels, derived in every instance from numerous observations, it will be seen that their mean places were confined to a range of about 2″. Unfortunately this high degree of constancy in the adjustments is not to be acquired until the instrument has been exposed for a long period to extremes of temperature, and frequently transported in rude vehicles along uneven roads. On commencing some subsequent operations in another quarter, it was unavoidably necessary to disturb the adjusting screws; a step which rendered the position of the reversing points as fluctuating as they had hitherto been constant. As to measurements made by instruments requiring the adjustment, on the spot, of the level, or even of the line of collimation, it is difficult to conceive how the most skilful observer can satisfy himself of their uniform accuracy.

Positions of the Reversing Points.

1829.		Right Index.	Left.	Mean.
		°	°	°
May 16.	At Great Whernside, on a rock....	59	76	67·5
June 8.	Reeth, on stone steps	61	74	67·5
10.	Muker, on a wall	62	75	68·5
12.	Kirkby Stephen, on a mantel-piece	61	75	68·0
15.	Hawes, on a window-ledge.........	62	75	68·5
17.	Ditto ditto	61	76	68·5
				Mean 68°

At Great Whernside, Shunnor Fell, and Bakestone Edge, the sector was placed on firm piles of stone set up within a yard or two of the signal. At the other stations the tripod of the theodolite, surmounted by a large level board, was made use of.

The observed refractions, with their deviation from the formula adopted in the calculation of the heights, are stated in the following table. With the exception of the two instances to which asterisks are prefixed, the quantities were all derived from reciprocal observation. The former are calculated from the vertical angle obtained at one station only; yet the contained arcs were so great, and the differences of altitude so well established, that they may fairly rank with the latter.

Stations.

Stations.	Arcs.	Refractions.	Error of Formula.
	′ ″	″	″
Great Pinch Yate and Water Crag	2 26	−20·5	+ 6·5
Keasdon and Shunnor Fell	3 3	− 1·5	− 9·5
——— Bakestone Edge......	3 6	−11·5	+ 1·
Great Whernside and Settronside	3 26	− 2·	− 6·5
Pickington Ridge & Bakestone Edge	3 46	− 8·	+ 1·5
Shunnor Fell..........................	4 20	− 1·5	− 2·5
Pickington Ridge & G^{t} Pinch Yate	4 31	− 4·5	− 0·5
Shunnor Fell and Nine Standards	4 54	0·	− 0·5
Water Crag and Shunnor Fell......	5 52	+ 8·	− 3·5
——— Pickington Ridge	6 25	− 5·	+12·5
——— Bakestone Edge	6 48	+ 0·5	+ 9·
Shunnor Fell and Pinch Yate	7 9	+23·	−11·5
——— Pickington Ridge	7 58	+12·5	+ 3·5
Great Whernside and Dod Fell ...	10 21	+33·	− 4·
Shunnor Fell and Whernside	10 23	+27·5	+ 1·5
Gt. Whernside and Bakestone Edge	11 56	+37·5	0·
Shunnor Fell and Pen-y-gent	12 54	+39·5	+ 3·5
——— Ingleborough ...	13 30	+45·	+ 1·
G^{t} Whernside and Ingleborough...	14 1	+46·	+ 3·
** ——— Whernside. ...	14 44	+58·	− 5·
** ——— Shunnor Fell...	15 2	+55·	− 0·5
——— Rumbles Moor	16 36	+65·	− 2·

Sum of arcs 10991″; *

Sum of refractions $({}^{+}450'' + {}^{-}54{\cdot}5 =){}^{+}396''$.

Mean refraction $\frac{396}{10991} = \frac{1}{27{\cdot}8}$ positive.

The indiscriminate adoption, throughout the calculations, of a positive refraction in one constant ratio of the arc, although nearly one-half of the observations indicate its negative quality, would evidently lead to irreconcileable discordances in the results. As the negative quantities diminish, and the affirmative ones increase with the contained arc, it is possible that the refraction may have been always positive, and nearly in the same proportion to the arc, but diminished by some unvarying quantity arising from local refraction or imperfection of the instrument. If we denote by $r'\left(=\frac{a}{n}-x\right)$ the observed refraction of the arc a, affected by the unknown quantity x, and represent by $t'\left(=\frac{c}{n}-x\right)$ the corresponding quantity for the *greater* arc C, then will the true refraction S of an arc b, equal to their difference ($=c-a$) be equal to $t'-r'$;

$t' - r'$; and $\frac{s}{b}$ the correct measure of the (constant) refraction in terms of the arc*. To insure success to the calculation, the smaller arc should be considerably inferior in magnitude to the larger one, and be derived from very numerous data. If we have given any number n of arcs with their respective observed refractions, each affected by the unknown error $-x$, the sum of the latter will contain $-x \times n$; which sum, divided by n, will give the true refraction *minus* $x \times 1$, (or equivalent of the *observed* refraction) of the nth part of the sum of those arcs.

The sum of the 10 first arcs is	″ 2509;	the sum of their refractions	″ 46 neg.
One-tenth	251;	one-tenth	4.6 neg.
The sum of the 9 last arcs is	7167;	the sum of their refractions	406 pos.
One-ninth	796;	one-ninth	45·1 pos.
Then, as the greater arc is	796;	and its *observed* refraction	45·1 pos.
And the lesser arc	251;		4·6 neg.
The *true* refraction of their difference	545″	will be equal to	49·7 pos.

The constant refraction being evidently $\frac{49{\cdot}7}{545}$, or very nearly $\frac{1}{11}$th of the contained arc, it follows that the difference between that proportion of either the greater or lesser arc and its observed refraction will be equal to the constant error of observation. Hence $\frac{796}{11} - 45''{\cdot}1$; or $\frac{251}{11} - {}^{-}4{\cdot}6 = 27''{\cdot}5$; whence the formula must be $\frac{\text{arc}}{11} - 27''{\cdot}5$.

On comparing the observed refractions with the quantities furnished by the formula, the deviations will be found, in almost every instance, to be not only trivial, but, what is equally conclusive of the justness of the rule, they are alternately positive and negative, without regard to the extent of the arc. The discrepancies exceeding 6″ are but four in number, and admit of considerable explanation. At Shunnor Fell the telescope has in all probability been pointed at the top of the wall

* On the same principle the index-error of a box-sextant may be conveniently found. Let a terrestrial object A lie between two others B and C, all the three, as well as the eye of the observer, being apparently in the same plane. Measure the arcs AB, BC, and (their sum) AC. From the latter subtract AB, leaving the correct value of BC, which will differ from its observed measure by the amount of the index-error.

on

on Keasdon in lieu of the base of the signal, of which it would interrupt the view,—a mistake that would tend to give a refraction proportionately too great. At Water Crag the depression of Pickington Ridge is noted in the field-book as an uncertain observation; the signal, owing to the heat and haziness, being tremulous and dim. From the same causes it was never once possible to bisect, even with the telescope of the theodolite, the signal on West Stonesfield Moor, at that time undestroyed. The difference of $11''\cdot5$ in the observed and computed refraction between Pinch Yate and Shunnor Fell will not appear extraordinary, when we take into consideration that the elevation of the latter must be in a great degree vitiated by the rare deviation of $12''\cdot5$ in the error of collimation from its mean value. Lastly: were the refraction between Bakestone Edge and Water Crag founded exclusively on the data of last year, it would accord within $6''$ of its assigned value.

The formula of refraction for the survey of the Dent Hills, deduced chiefly from small arcs, was confessedly inapplicable to those of considerable extent.

The formula of the Swaledale survey may however be substituted with singular success, and will be found to satisfy the observations on arcs of both descriptions. On the other hand, it is remarkable that the latter formula should differ materially from the equally correct one made use of in computing the altitudes of the Wensleydale Fells. The three surveys were nevertheless carried on all about the same time of the year, and the observations took place nearly at the same hours of the day. In fact, the only circumstances peculiar to the Wensleydale survey which could possibly account for the difference in its refractions, were the almost total absence of frost, and the excessive humidity of the atmosphere.

If we may not ascribe the constant error of $27''$ to the influence of local refraction, it is quite certain that it cannot justly be attributed to a false estimate of the cylindrical error of the sector. Not only do the most recent experiments indicate the quantity already assigned to be too great, but every comparison of the measurements by the sector with the corresponding ones by the great theodolite of Ramsden confirms the exaggeration. A few of the zenith distances observed at Ingleborough by the two instruments, corrected for their respective constant errors, and reduced to the level of the ground, are cited in the next table.

By

	By Ramsden's Theodolite.	By the Sector.	Differences.
	° ′ ″	° ′ ″	′ ″
The Calf..........	90 13 42	90 12 23	−1 19
Shunnor Fell.....	90 8 7	90 6 58	−1 9
Great Whernside	90 9 27	90 8 48	−0 39
Whernside.......	89 56 55	89 55 32	−1 23

The *first* column of the subjoined register of the measurements by the horizon-sector contains the names of the hills of which the *ground* at the base of the signal, unless otherwise expressed in italics, had been observed;—the *second* the mean of the readings by the two indices, of the elevation or depression, each corrected for the constant error of the instrument and the deviation of the bubble from its reversing points; the further reductions, requisite to obtain the differences of level given in the *third* column, being for the height of the eye, the curvature of the earth, and the constant refraction of $\frac{\text{arc}}{11} - 27''\cdot5$. The *last* column, given merely as a test of the goodness of the observations, exhibits the deviation of the error of collimation of each (pair of) observations, from the mean error of the whole.

At Pickington Ridge.

June 6th, 1829. Height of Eye 3·5 feet.

A very cold and clear afternoon, with a steady dry wind from the E.N.E.

	° ′ ″		Feet.		″
The Hoove..............	0 6 33	depr.	36·3	lower.	3·5
Calvey....................	0 48 19	...	260·2	...	2·5
Dod End.................	0 22 37	...	175·2	...	0·5
Satron Hangers........	0 22 23	...	81·6	...	2
Gibbon Hill.............	0 37 16	...	75·6	...	10
Grinton Grits...........	0 55 33	...	178·6	...	4·5
Robincross Hill........	1 8 19	...	448·9	...	10
Whitfield Hill..........	1 0 55	...	506·4	...	2·5
Blake Hill...............	0 1 58	...	6·4	higher.	2
Water Crag	0 25 46	elev.	...	...	6
————	0 25 50	...	331·9	...	5·5
Shunnor Fell	0 30 56	...	494·2	...	7·5
Great Pinch Yate......	0 4 38	...	59·1	...	2·5
Rogan's Seat...........	0 29 5	...	348·0	...	2·5
Nine Standards Hill...	0 11 6	...	316·4	...	2
Bakestone Edge........	0 7 6	...	64·4	...	4
Brownsey................	0 2 50	...	38·2	...	0

At

At Harker Fell.

June 6th, 1829. Height of Eye 4·5 feet.

A beautifully clear evening, but unseasonably cold, and without perceptible moisture in the air.

	°	′	″		Feet.		″
Calvey....................	0	21	26	elev.	70·7	higher.	3·5
The Hoove	0	28	4	...	291·1	...	6·5
Great Pinch Yate	0	52	46	...	385·7	...	2·5

At Great Pinch Yate.*

June 8th, 1829. Height of Eye 3·5 feet.

A cold, clear afternoon, with a strong breeze from the north-east, bringing at intervals gusts of mist with small rain. Finally heavy rain not reaching to the valleys.

	°	′	″		Feet.		″
The Hoove	0	20	46	depr.	93·0	lower.	1
Calvey	1	9	14	...	316·0	...	3
Holgate Pasture	0	53	54	...	483·7	...	2
Robincross Hill	0	47	50	...	508·6	...	5
Pickington Ridge	0	10	10	...	59·6	...	8·5
Gibbon Hill	0	20	34	...	134·8	...	4
Grinton Grits...........	0	29	54	...	235·3	...	0·5
Brownsey	0	10	37	...	20·6	...	1·5
Blake Hill...............	0	13	54	...	53·7	...	1
Satron Hangers.........	0	24	22	...	139·4	...	4·5
Bakestone Edge	0	2	12	...	5·9	higher.	5
Water Crag	1	1	2	elev.	273·8	...	2·5
Rogan's Seat............	0	57	36	...	290·8	...	5
Shunnor Fell...........	0	30	55	...	438·6	...	12·5

At Water Crag.

June 10th, 1829. Height of Eye 3·75 feet.

An excessively hot, calm and cloudless afternoon. Considerable haziness and ebullition in the atmosphere, especially in the direction of the sun. Thunder-storms in Cumberland on the same day.

	°	′	″		Feet.		″
The Hoove..............	0	53	9	depr.	368·6	lower.	5·5
Great Pinch Yate......	1	5	49	...	274·6	...	5
Brownsey	1	10	22	...	294·0	...	6
Pickington Ridge........	0	33	1	...	336·8	...	1
Highest Standard, *top*...	0	7	12	...	40·8	...	5·5
Blake Hill	1	18	11	...	325·6	...	6·5
Satron Hangers	0	52	22	...	416·8	...	2

* Sometimes called *The Surrender*, from the celebrated mine of that name, situated on the west side of the crown of the fell.

Bakestone

	°	′	″		Feet.		″
Bakestone Edge	0	31	27	depr.	269·0	lower.	4·5
Rogan's Seat	0	5	34	elev.	15·0	higher.	4
Hugh Seat	0	7	8	...	135·0	...	2·5
Shunnor Fell	0	12	2	...	...	...	2
————	0	12	7	...	158·7	...	0

At Keasdon.

June 11th, 1829. Height of Eye 4 feet.

Time of observation about noon. Very hot, and nearly cloudless, yet tolerably clear. Violent thunder-storms with torrents of rain in Westmoreland.

	°	′	″		Feet.		″
Blake Hill..................	1	15	30	elev.	224·8	higher.	2·5
E. Stonesdale Moor.....	0	56	48	...	227·5	...	4
Hugh Seat................	1	10	53	...	691·1	...	5
Shunnor Fell	2	9	33	...	713·6	...	1
Dod End..................	0	25	10	...	45·3	...	0·5
Bakestone Edge	0	49	18	...	283·7	...	4
W. Stonesdale Moor ...	0	36	48	...	......	...	0

At The Nine Standards Hill.

June 12th, 1829. Height of Eye 3·5 feet.

About noon. Bright to the east, cloudy to the west; sultry, with brief but heavy showers.

	°	′	″		Feet.		″
E. Stonesdale Moor ...	0	44	9	depr.	308·9	lower.	2·5
The Tail Brigg	2	49	30	...	375·5	...	0·5
Highest Standard, *top*...	1	0	44	...	21·8	...	14·5
Water Crag...............	0	1	26	...	16·6	higher.	2
Shunnor Fell	0	17	21	elev.	......	...	2·5
————	0	17	12	...	175·0	...	2
Hugh Seat	0	28	4	...	155·9	...	2
Pillar Hill	0	9	52	...	85·1	...	1·5

[To be continued.]

XVIII. *Statement respecting the Discovery of the new Species of Swan, named* Cygnus Bewickii *by Mr. Yarrell. By* A Correspondent.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

ALLOW me, through the medium of your Journal, to make a few remarks upon a paper by Mr. Yarrell, in the last published part of the Transactions of the Linnæan Society, descri-

describing a new species of swan, named by him *Cygnus Bewickii.* These remarks are rendered necessary, in order to supply some deficiencies in the paper above named, and that justice may be done to the proper discoverer and original describer of this new swan, Mr. R. R. Wingate of this town, who is perhaps one of the best practical ornithologists of the present day, and was for many years the intimate friend and acquaintance of the highly-talented individual, whose name Mr. Yarrell has so properly connected with this valuable addition to our *Fauna.*

It was in the February of 1829, that the original specimen of *Cygnus Bewickii* was shot near Haydon Bridge in the county of Northumberland; it came into the possession of the Literary and Philosophical Society here, and was sent to Mr. Wingate to be preserved for the Newcastle Museum. Upon examination he immediately pronounced it to be of a species distinct from the common wild swan or hooper; and fortunately another specimen of the same was already in the possession of the Messrs. Hancock of this town, the peculiarities of the anatomical structure of which had attracted their attention, and which upon investigation was found to confirm the views originally taken of the species. So noble a discovery was of course much talked of by the lovers of natural science here; and a gentleman, who is one of the ornithological curators of the Natural History Society, being in correspondence with Mr. Yarrell, mentioned the circumstance, and pointed out some of the distinctions observed in the new species. To this letter he replied that the differences pointed out, might arise from age, sex, or accidental circumstances, or even error of observation, more likely than that a new species of a bird so conspicuous as this should have remained so long undiscovered. Notwithstanding the weight of such an opinion from Mr. Yarrell, Mr. Wingate was not deterred, being confident of his discovery, but proceeded to draw up a short notice of the new bird for the Natural History Society of this town. In the mean time a new light had dawned upon Mr. Yarrell, as he subsequently writes that he was now convinced the bird was of a new species, and proposed the name of *Bewickii* for it; at the same time liberally offering either to supply Mr. Wingate with such materials as he had been able to collect, or that Mr. Wingate should supply him, in order that a proper description might be drawn up and published. In reply, he was informed that Mr. Wingate had already drawn up a notice of the bird, and that Mr. Selby, one of the Vice-Presidents of the Society, whose name stands foremost amongst the ornithologists of Britain, had undertaken to describe it.

Mr. Wingate's notice was read to the Natural History Society on the 20th of October 1829, and Mr. Selby's description on the 16th of February 1830: both these papers, with beautiful illustrations from the pencil of Mr. Selby, will appear in the first part of the Transactions of the Natural History Society, about to be published.

Mr. Yarrell's high scientific acquirements are well known and generally appreciated; nor is there any wish to detract from his merit in this particular instance, in tracing out examples of peculiar structure in individuals of this genus which had been observed by himself and others at different periods: at the same time it must be acknowledged that Mr. Wingate's skill as an ornithologist is placed in a very favourable point of view by the discovery, his acute observation having enabled him to point out a new species, from the first specimen which came under his notice. I am, Sir, yours, &c.

A Member of the Natural History Society of Newcastle-upon-Tyne.

Newcastle-on-Tyne, June 14, 1830.

XIX. *Results of an extensive Re-examination of the Narcissean Group of Plants.* *By* A. H. Haworth, *Esq. F.L.S. &c. &c.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

As the valuable purchase of the Linnæan Herbarium by the Linnæan Society of London has enabled me to make a few useful remarks on, and additions to, my *Narcissorum Revisio*, I avail myself of the opportunity so afforded, and of your excellent Miscellany, for such purposes.

Having, as far as possible to me, extensively re-examined the Narcissean group of plants; indeed, very nearly the whole, (except a few species which I studied in 1819 in the garden of the Horticultural Society of London, but was desired not to publish,) I am pretty confident as to the correctness of the following statements.

Ad Narcisseas Additamenta quædam.

Genus, Ajax Nob. *Narciss. Revis.* 115.

lobularis. A. (Truby, 6-lobed Daffodil.) Corollæ laciniis luteis tubo obconico exactè duplò longioribus: coronâ perluteâ patulâ sexlobatâ (lobis integris), lacinias 3 lineas superante.
1.

Habitat prope Truby in comitatu Varvicensi in pratis spontaneus.

spontaneus. Communicavit viventem, cum flore, amicus Rev. Gul. T. Bree, ineunte Aprili 1830.

Obs. Prope A. obvallarem *Salisb.* cui maximè affinis, certè locarem. Differt satis, flore omni parte longiore. Novam speciem constituit: et forte affinior *A. spurio* Nob. *in Narciss. Revis.* 115. *et Synops. Succ. App.* 327, cum coronæ lobis longissimè integrioribus.

Obs. Folia florendi tempore pedalia erecta firma obtusa 5 lineas lata, infernè inflexo-canaliculata, extùsque obtusè carinata; supernè planata; undique striatula, paululùm glaucescentia, seu magìs viridia quàm in affinibus proximis. *Scapus* folia 2-3 uncias superans ordinarius seu paululùm compressus, acutè anceps, et cum foliis concolor. *Spatha* tubum subæquans. *Tubus* ferè campanularis (externè) perluteus, basi virescens. *Corollæ* laciniæ basi imbricantes: *exteriores* ovato-subacuminatæ patulo-subincurvantes: *interiores* lato-lanceolatæ subtortuosìm obliquè flexulæ. *Corona* lævis ore patulo, lobis sex conspicuis rotundatis rectìmque triplicatulatis, ferè integerrimis; *intùs* ad lentem aliquantillum rugosiusculis. Cætera non examinavi.

It may in this place be observed that the two double varieties given to Ajax telamoneus in *p.* 115 of *Narciss. Revis.* seem rather to belong to Narc. major of *Bot. Mag.* 51.

moschatus. A. (The white-flowered.) Ajax albus *Nob. Narciss.*
2. *Revis.* 117.—Ajax patulus *Salisb. in Hort. Trans.* 1. *p.* 348. est

Narcissus moschatus *Linn. Sp. Pl. ed.* 2. *p.* 415: et ejus *Herbarii.*—Narc. moschatus, δ, *Kerr in Bot. Mag. t.* 1300.—Narc. candidissimus *Redouté, Liliac. t.* 188.

tortuosus. A. (The greater white.) Ajax moschatus *Nob. Nar-*
3. *ciss. Revis.* 118. (exclus. synon. primo). Ajax longiflorus *Salisb. in Hort. Trans.* 1. *p.* 349.—Narciss. tortuosus *Nob. Misc. Nat. p.* 179.—Narciss. moschatus, α, *Kerr in Bot. Mag. p.* 924.

Obs. The Herbarium of Linnæus proves to me that the last, not the present, species, is the plant he named *N. moschatus*; I have therefore restored it, and preserved that of *tortuosus*, which I originally gave to the present plant, and which aptly alludes to its very tortuous petals.

cernuus. A. (The drooping white) spatha uniflora, nectario
4. cylindrico crispo 6-fido petalis ovatis obliquis longiore, flore cernuo. *Roth Catalect. Bot. fasc.* 1. *p.* 43.

Obs. I have never seen this plant in a *single* state (for it must be distinct from the two preceding species), but a root of a very beautiful double-flowering variety of it, from my friend the Rev. Mr. Ellicombe of Bitton Vicarage, near Bath, bloomed finely with me last March.

bicolor. A. (The broad-leaved.) Ajax bicolor *Salisb. in Hort.*
5. *Trans.* 1. *p.* 346, et *Nob. in Narciss. Revis.* 119.

Obs. This is the true *Narcissus bicolor* of Linnæus, as appears by the specimen yet well preserved in his Herbarium, but the leaf is not that of a flowering bulb, but rather of a younger part of the plant. My own Narciss. bicolor *Linn. Trans. v. 5. p.* 244, and of *Bot. Mag. t.* 1187, became Ajax larifolius in my *Narciss. Revis. p.* 119, in the year 1819.

Genus, QUELTIA Nob. *in Narciss. Revis. p.* 121.

capax. Q. (The straw-coloured.) Narcissus calathinus *Redout.*
6. *Lil. t.* 177, nec aliorum.

Obs. I have not seen this plant in a single state, but possess a beautiful double-flowering variety of it, from my friend the Rev. W. T. Bree, of Allesley, near Coventry; which is both well figured and well described in Parkinson's famous *Paradisus Terrestris, t.* 107. *f.* 4; and, until the present season, was one of the missing or lost hardy bulbous beauties of that faithful writer, and of the parterres of our forefathers two hundred years ago.

Obs. In this place it is convenient to remark that *Philogyne minor* of my *Narciss. Revis. p.* 137, was probably only a small plant of Philog. heminalis of *Salisb.*, and that the plant well known by the name of Queen Anne's Jonquil, is doubtless a double variety of a plant more recently figured in the *Botanical Register, t.* 816, by the name of *Narc. gracilis,* and which appears to be the Narcissus juncifolius luteus *albicantibus* lineis distinctus, of *Park. Parad. p.* 94. No. 10, but which, till lately re-introduced, appears to have been long missing in our gardens.

It may be added, that the very intelligent foreman of Mr. Young's Nursery at Epsom, pointed out to me in April last, a lesser but also double variety of Queen Anne's Jonquil, which before I had not seen.—For a list of the lost hardy bulbs of Parkinson's days, see my Notice in the *Gardener's Magazine* for July 1830.

Genus,

Genus, HERMIONE Nob. *in Narciss. Revis. p.* 137.

Jasminea. H. (The Jasmine-like) sub-5-flora: corollæ elegan-
1. tissimæ niveæ, laciniis lanceolatis stellatis non imbricantibus, coronâ erosulâ 5-plò longioribus.

Hermione Jasminea *Salisb. in Hort. Trans. v.* 1. *p.* 360.—Herm. papyratia, β, *Nob. in Narciss. Revis. p.* 143, cum observatione dubitanti.

papyratia. H. (The paper-white) sub-10-flora: corollæ niveæ
2. elegantis, laciniis sub-ovato acutis imbricantibus, coronâ erosâ 3–4-plò longioribus.

Hermione papyratia *Nob. Narciss. Revis.* 143.—Narcissus papyratius *Kerr in Bot. Mag.* 94.—Narciss. unicolor *Jard. de Malmaison, tab.* 26.

præcox. H. (The early-flowering) sub-10-flora: corollæ la-
3. ciniis elliptico-lanceolatis basi imbricantibus pallidè sulphurascentibus, coronâ citrinâ subquadruplò longioribus: stylo coronam erosam æquanti.

Hermione stylosa *Salisb. in Hort. Trans. v.* 1. *p.* 360. Herm. italica, α, dubitata, *Nob. Narciss. Revis.* 114. Narcissus præcox *Tenore*, Fl. Neapolitana, *t.* 3. Narcissus italicus *Kerr in Bot. Mag.* 1188.—N. sulphureus maj. *Park. Parad. p.* 79.

Obs. Sub dio initio Martii floret.

tenuiflora. H. (Slender-flowering Italian) subquadriflora: co-
4. rollæ laciniis sordidè albis lanceolatis stellatis non imbricantibus, coronâ minutâ luteâ sublacerâ incurvo-erectâ 5-plò longioribus.

Hermione italica, β, tenuiflora *Nob. in Narciss. Revis.* 114.—N. sulphureus minor *Park. Parad. p.* 79.

Obs. This flowers late in April, after the flowers of the preceding are past away; and is the slenderest flowered true Hermione yet known to me. The double and semi-double varieties hitherto proposed doubtfully under it, have not yet come fairly under my examination; and they probably belong to a distinct species from Cyprus, which at present I would designate *H. Cypri.*

Fearful of taking up too much room in your valuable pages, it may be thought perhaps by some, that I have been too brief; but long experience has convinced me very satisfactorily, that a really good specific diagnosis is better, less perplexing, and more easily and even more certainly understood,

stood, than laborious descriptions of a thousand words. The former at once *defines* the species; but the latter distract by detailing the most insensible variations.

I remain, Gentlemen, very respectfully,
your old correspondent,

Chelsea, May 1, 1830. A. H. Haworth.

Postscript.—Please to notice the following errata, &c.

For "Cotyledones 2, parvæ," in my last communication, vol. viii. p. 106, l. 28, read "Cotyledones nullæ." For I have just seen two germinating species of Mammillaria without any.

In page 107, line 11, for "Cotyledones nullæ," read "Cotyledones duæ." For I have also very lately perceived two very minute apical ones, on an apparent species of Cereus. And on mongrels purposely raised between Cereus speciosissimus and Epiphyllum Phyllanthoides, I have likewise very recently found two apical conspicuous connate and cordato-ovate fleshy Cotyledons; and have preserved the whole in spirits.

In my communication for October 1825, page 177, l. 32, for "frequens" read "sequens"; for

September 1829, p. 302, l. 20, for "erectus" read "erecta;"

February 1830, p. 108, l. last but one, after "reptantibus" add "ramis;"

February 1830, p. 114, l. last but one, for "plantis" read "plantæ".

To the above may be usefully added, that DeCandolle found two very minute basal Cotyledons on *Melocactus communis*, and two small apical ones on his *Echinocactus cornigerus*. And hence Echinocactus becomes a subgenus only.

A. H. H.

XX. *On the Dying Struggle of the Dichotomous System. By* W. S. MacLeay, *Esq. M.A. F.L.S. In a Letter to* N. A. Vigors, *Esq. F.R.S.*

[Continued from p. 57.]

Dr. Fleming, however, has other and equally powerful reasons for disputing the law of continuity in the construction of organized matter. Because some planets in our system have moons, and others not; because Saturn has a ring, and because nearly one-half of the comets move from west to east; therefore there is no transition from one form of organized matter to the other. Q. E. D. So also, "If we arrange the elementary bodies of which this globe is composed in a table, according to their saturating power or atomic value, the eye will not fail to perceive a series of *coincidences* or *starts*, more or

or less extensive, from hydrogen at the one extremity to uranium at the other;" therefore there is no transition from one form of organized matter to the other. Q. E. D. Nay, he favors us with a *reductio ad absurdum* of equal value: to wit; Let this law of continuity operate on the elements in their mutual relations, then there cannot exist such a class of bodies as chemical compounds: but these chemical compounds do exist—*ergo*, there is no transition from one form of organized matter to the other. Q. E. D.

But although it be difficult to comprehend by what legerdemain moons and hydrogen are, *in the above manner*, brought to bear on the matter, I would ask how Dr. Fleming comes, to know that *in the universe* there are not planets with one two, three, or even a hundred of moons, or other bodies with rings, besides Saturn? and, above all, I would ask how he comes to know that minerals do not approach each other more or less in nature? He is here at direct issue with one of the most distinguished members of the French Institute, M. Ampère, as great a mathematician as chemist, who has published a *Classification Naturelle pour les Corps Simples*," and proved, that " les corps sont tellement *coordonnés l'un à l'autre*, qu'ils ne forment non plus une série mais *un cercle!*" As to the Doctor's *series of coincidences or starts*, I can only say that such an extraordinary coincidence is sufficient to make any body start, and that I trust he will explain it when he publishes his Dictionary of Synonyms.

The law of continuity, as it relates to forms of matter, may truly be proved possible in itself, and, in the next place, to exist in nature. The critic, however, really does not understand what it means, and reminds me of an equally bright acquaintance, who inquired, how there could be a law of continuity in the gradation of structure, unless all created beings were glued together. His ideas of continuity, like Dr. Fleming's, were taken probably from a string of mules, where one's head is tacked to the other's tail; that is, continuity with respect to space. Continuity in gradation of structure has however nothing to do with space or time. Matter, with respect to space, is capable of incontinuity, but with respect to gradation of form, it is as clearly capable of continuity. I will try to explain this truth to Dr. Fleming; and for this purpose let us return to the above-mentioned beautiful Grecian temple, the kirk of Flisk; and let us suppose it to have for its neighbour a sublime specimen of the Gothic in the parish-kirk of Frisk. Let us suppose, that between these two different kirks there is a transition made from one form to the other by an infinite number of intermediate kirks, passing from

from pure Grecian to the pure Gothic architecture. The continuity, wheresoever as to space the kirks intermediate in structure may be placed, will be perfect so far as relates to the gradation of form. And yet there must ever be some difference between the two structures nearest each other in form; for if no interval exists, then these two must have the same structure, and one of them will thus produce no effect in continuing the chain of structure. In this kind of continuity, therefore, intervals between different forms *are absolutely necessary;* and if they do not exist, there is only one form. In space or time, which afford the only continuity that Dr. Fleming can comprehend, an interval is impossible; and their continuity depends on this impossibility. On the other hand, continuity in gradation of structure depends on the existence of intervals; but requires, in order that the gradation be more distinct, that these intervals be extremely small and numerous. If only one mean be interposed between two extremes, there will be two chasms but no *saltus*, and the three objects will be in continuity. Augment the number of various intermediate objects, and you only get the chasms more numerous and the continuity more perfect. To chatter therefore about the innate impossibility of the law, is absurd: the only question for us now to examine, being, whether such a continuity as I have described can be shown to exist in Nature.

I think I have proved this in my Analysis and Synthesis of Petalocerous *Coleoptera*. You, my dear Vigors, have proved it in Birds; and what the Linnæans call natural genera, such as *Rosa* and *Erica*, are likewise all proofs of it: so that if continuity manifestly holds good in these particular parts of the Creation, which have been carefully examined, it may hold good in all. True it is that Nature does not always proceed *pari passu*. In the Linnæan genus *Psittacus*, a group of very limited structure, the chain is composed of an immense number of links; whereas, in *Pachydermata*, a group presenting a very wide range of structure, the number of links is comparatively small. Still there is continuity manifest in both; the difference depending merely on the relative distance between some two contiguous forms in each. Chasms in the chain may be numerous and small, as in *Psittacus*, or few and wide, as in *Pachydermata;* but an *hiatus* is not synonymous with a *saltus*.

Some years ago, in a paper in the fourteenth volume of the Linnæan Transactions, I stated it as an undoubted fact, that *hiatus* or chasms are every where in nature presenting themselves to the view; and I think I have now satisfactorily explained

plained how the more numerous they are they produce the more continuity. " But this truth by no means contradicts the Linnæan maxim, that no *saltus* exists in nature, although such has been esteemed its effect by certain naturalists, who have been in the habit of taking the words *hiatus* and *saltus* as synonymous terms. Thus the series of the *Systema Naturæ* and of the *Regne Animal* is not natural where the *Cetacea* intervene between quadrupeds and birds, but is perfectly consonant with nature where the tortoises are made to follow these last. In the first case there is a *saltus* or leap from quadrupeds to birds over a group totally dissimilar to the latter; there is, in short, an unnatural interruption of the law of continuity, which shocks not merely the naturalist but the ordinary observer. In the other case there is only an *hiatus* or chasm, which the discoveries of a future day may fully occupy."

Having thus, I think, established the truth of the law of continuity as well as of an unity of plan in the Creation, I arrive at the cold, unfeeling sneer on the venerable and excellent naturalist, whom Dr. Fleming, ever equally accurate, calls Lamark. I am so far removed from the scientific world that I know not whether Lamarck be alive or dead; but I revere him if still on earth, and respect his memory if he has ascended to a better place. Time has only shown me more and more the truth of what eight years ago I said of him. " His peculiar and very singular opinions have never gained many converts in his own country, and I believe none in this. They are indeed only to be understood by those who are already supplied with the means of refuting them; so that the mischief they may have occasioned being comparatively null, we may be permitted to assign due praise to the labours of Lamarck, as being those of the first zoologist France has produced; as being those of a person, whose merits in natural history bear much the same relation to those of M. Cuvier, that the world has been commonly accustomed to institute between the calculations of the theoretical and the observations of the practical astronomer."

Dr. Fleming, by the way, seems to hint that I borrowed the distinction of affinity and analogy from Lamarck. But this only proves that he reads as he reasons. Lamarck says, " On distingue les rapports en ceux qui appartiennent à differens êtres comparés, et en ceux qui ne se rapportent qu'à des parties comparées entre des êtres differens." Now the first of these kinds of *rapports* may be either relations of affinity or of analogy, for both affinity and analogy present resemblances between different objects compared with one another; and the second kind of *rapports* I shall speedily explain as having no connexion with relations of analogy. There may be good

reason to doubt whether Aristotle did or did not make the true distinction between relations of affinity and analogy, as I have shown in a paper lately most shamefully misprinted in the Linnæan Transactions, vol. xvi. p. 9.; but it is indisputable that Lamarck never did make the distinction. Lamarck describes three kinds of "rapports entre des organizations comparées," and two kinds of "rapports entre des parties semblables ou analogues." All the three first kinds of "rapports" appear to be relations of affinity. It is indeed possible that true relations of analogy may be *confounded* with relations of affinity under the second of these three, which is "celle qui embrasse les rapports entre des masses d'animaux differens comparées entre elles." Vol. i. p. 354. But whether this be so or not, the "rapports entre des parties semblables ou analogues," however these words may jingle in the ears of Dr. Fleming, have, as Lamarck has explained them, nothing whatever to do with what I term relations of analogy. Of these "rapports" he describes, as I have said, two kinds, viz. "rapports particuliers entre des parties non modifiées," and "rapports particuliers entre des parties modifiées:" in other words, the relations *in point of value*, as a groundwork of distinction, between different systems of organs, such as those of digestion, respiration, circulation, &c., and the relations *in point of value*, as a groundwork of distinction, between different forms of the same organ as they exist in different groups. The study of the first kind of these relations is of use to point out to us whether, in the variation of animals for instance, we ought to lay most stress on the organs of digestion, like Linnæus, or on the form of their eggs, like Sir Everard Home. The study of the second kind of relations is of use to point out to us whether, in the arrangement of animals for instance, we ought to lay most stress on the variation of the structure of the eye in *Vertebrata* where it is perfectly formed, or among *Mollusca* where it is imperfect. Surely neither of these two last relations are relations of analogy. Yet this Dominie, who cannot understand Lamarck, has the impertinence to scoff at him!

M. Virey out of national jealousy, as Dr. Fleming from other feelings, has attacked me on this head. They have both impotently endeavoured to fix upon me the charge of plagiarism, with respect to the distinction of relations of affinity from those of analogy. I have however repeatedly stated that Linnæus, Pallas, and Desfontaines, and even Aristotle himself, have all mentioned certain analogies in nature as distinct from affinities, before I was born. They have mentioned the existence of this distinction in particular cases,

but

but I first pointed out its nature and its general application, and called the attention of naturalists to the subject.

Dr. Fleming says, that the distinction between these two relations is only respected by *me* (for, as I have shown, Lamarck never made it) when they suit my views. This is to a certain degree true; but let us examine the full value of the remark, and we shall find that it is a proof of my respect for nature. Let us take his definition of relations of affinity and analogy which he fathers off upon me, but to which he nevertheless appears to give his full consent. *Relations of affinity*, says he, are relations of resemblance between different objects compared with each other; and *relations of analogy* are the relations of particular parts of different objects. Why, if this be all the distinction, there is in reality none; the last kind being clearly involved in the first—a resemblance between parts being only a partial resemblance between the wholes. How then is this confusion between the two relations to be prevented? By applying another and most necessary condition to relations of analogy, namely, their *parallelism*. And now Dr. Fleming will understand the reason why I only respect the distinction he makes between relations of affinity and analogy, when the latter *suit my views* of their necessary parallelism. I will repeat here for him, what I long ago said on the subject in the Linnæan Transactions: "The *theoretical* difference between affinity and analogy may be thus explained. Suppose the existence of two parallel series of animals, the corresponding points of which agree in some one or two remarkable particulars of structure. Suppose also that the general conformation of the animals in each series passes so gradually from one species to the other, as to render any interruption of this transition almost imperceptible. We shall thus have two very different relations, which must have required an infinite degree of design before they could have been made exactly to harmonize with each other.

"When therefore two such parallel series can be shown in nature to have each their general change of form *gradual*, or, in other words, their relations of affinity uninterrupted by any thing known; when moreover the corresponding points in these two series agree in some one or two remarkable circumstances, they afford relations of analogy, and there is every probability of our arrangement being correct. It is quite inconceivable that the utmost human ingenuity could make these two kinds of relation tally with each other, had they not been so designed at the Creation."—See also Linn. Trans., vol. xiv. *note*, p. 52.

If naturalists did but study the works of MM. Fries and

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Agardh on this subject, they would not fall into absurd and fantastical comparisons, which rest on the same foundation as the ancient analogy detected between the moon and a green cheese, on account of both being round. The learned Mr. Kirby, for instance, in his "Introduction to Entomology," has written in a most flattering manner of my distinction between relations of affinity and analogy; but it grieves me to be obliged to confess that he appears not to understand it, and that his mistake principally proceeds from his forgetting the necessity of parallelism between different relations of analogy. If this respectable naturalist will study the works of Fries, and a little work entitled *De Plantarum præsertim Cryptogamicarum Transitu et Analogiâ Commentatio*," published by Theophilus Gulielmus Bischoff at Heidelberg in 1825, and will then praise me, I shall be gratified by his praise; at present I must say I feel that I do not deserve it, and unmerited approbation is a poor recompense for being made to patronize or father notions that I have no wish to lay claim to.

[To be continued.]

XXI. *Narrative of an Excursion to the Summit of the Peak of Teneriffe on the 23rd and 24th of February 1829.* By ROBERT EDWARD ALISON, *Esq.*

[Continued from p. 30.]

AFTER leaving on our left a steep mountain of pumice, called *Montaña Alta*, we passed *La Estancia de la Cera* and *La Cueva de la Machoura*, and entered the *Cañadas del Pico*, the thermometer standing at 50°·5. The Cañadas is an immense plain of white and yellow pumice, extending round the Peak from W.S.W. to E. by N. forming part of an ellipsis of seven or eight square leagues in extent. The surface is 8957 feet * above the level of the sea; and rather towards one side of this plain, in lat. 28° 17′ N., and in lon. 16° 39′ 45″ W. rises the Peak to the further elevation of 3231 feet.

At 10^{h} 30^{m} A.M., thermometer standing at 49°, we passed a mass of porphyritic rocks called *La Gayeta*, and afterwards some of a similar character, called by the guides *La Estancia de Juan Benitiz*. Shortly afterwards a thick mist swept across us,

* This elevation was ascertained by M. Mouneron by levelling, and afterwards confirmed by Humboldt by barometrical admeasurement, calculated by the formula of Laplace. Most of the elevations which I have given here were very kindly furnished me by Dr. Don Domingo Saviñon of Laguna, a physician, who by his various scientific attainments is an honour to his profession. This gentleman has a collection of valuable observations respecting the physical history of Teneriffe, which, it is to be hoped, he will at some future period give to the world.

us, which lowered the thermometer to 44°·5: we at the same time passed a small extinct volcano called *Montaña Negra*, or more generally known by the name of *Los Gorros*; in it are several caves, which the men who supply Santa Cruz and Orotava with snow, use as ice-houses, by filling them with snow, which they collect at the foot of the Peak at certain seasons of the year: when snow cannot be collected there, they go up to a cave, which is 2131 feet higher up. For four baskets of snow, which is a mule-load, they obtain at Santa Cruz a sum only equal to 13*s.* 4*d.* after undertaking a journey of nearly 80 miles, reckoning from their own house and back again. These men likewise act as guides to those who visit the Peak, and are generally found both faithful and obliging.

At twelve o'clock we arrived at the foot of the Peak, and halted for a few minutes. The surface was composed of reddish-coloured pumice, studded over with large blocks of *grünsteinic* lava of a grayish-green colour, mixed with crystals of felspar, masses of common obsidian, and large bushes of mountain broom. We found the temperature of the air to fluctuate considerably, according as the wind blew away the surrounding vapour. When we first arrived, the thermometer stood at 44° in the shade; but it shortly afterwards rose to 50° in the same situation, and to 57° where it was indirectly exposed to the influence of the sun. A small surface of æther, ·25 of a line in depth, and sheltered from the sun and wind, evaporated in rather less than three minutes; and the bulb of a thermometer, covered with silk and kept moist, lowered the mercury three degrees in the same time. When I ascended the Peak on the 13th of October 1827, the same experiments had different results. A similar quantity of æther, and at the same spot, evaporated in about one minute and a half; and the thermometer, enveloped in the same manner, fell 4°·5 in less than three minutes. I found the boiling point of water to be from 189°·5 to 190°, and strong Havanna rum boiled at 175°; in the town of Orotava (1040 feet above the sea) water boiled at from 209°·25 to 209°·5. When I ascended the Peak on the 13th of October 1827, water boiled at the same spot at 188°·5.

We began to mount the Peak by a very steep ascent over beds of small yellowish-coloured pumice, between two embankments or currents of lava, which had separated from the general mass, called Mal Pais, situated at Alta Vista Arriba, 1664 feet above the foot of the mountain. These currents are not in connected masses, but consist of immense blocks of various sizes and forms; and different parts of the same current appear to have undergone various states of combus-

tion [?];

tion [?]: some are an obsidian of a jet black colour, possessing internally a shining vitreous lustre, breaking with a conchoidal fracture, and translucent at the edges; others are a brownish porphyritic lava mixed with large crystals of felspar, partly destroyed by the action of fire; others have an earthy appearance, and although cellular, are hard and heavy; and towards the upper part of the stream were several blocks of phonolite of a greenish gray-colour.

After a fatiguing but not a difficult ascent, which took us three quarters of an hour to accomplish, we arrived at a part of the Peak which is 9930 feet above the level of the sea, called *La Estancia de los Ingleses de Abaxo,* the lower resting-place of the English: the pumice here forms a tolerably level surface of a few hundred feet square; towards the N.N.E. side of it are scattered several large blocks of obsidian; under the lee of one of them we lighted a fire of dried mountain broom, and piled up some stones to form a shelter from the wind, which was then blowing a hurricane.

One hundred and twenty-eight feet above this Estancia, is another called *La Estancia de los Ingleses de Arriba,* the upper resting-place of the English, and is to be noticed as the highest point at which the *Spartium nubigenum* is to be found; after inspecting it, I think this place affords better shelter in summer to a *small* party of visiters to the Peak, than the one below.

After taking some refreshment, we resumed the ascent with the intention of observing the temperature of the summit that evening, and likewise next morning; the acclivity became more and more difficult, the pumice frequently gave way beneath our feet, and in many places the frozen state of the snow gave us considerable annoyance from the extreme difficulty of climbing up it.

In an hour we arrived at *Alta Vista Arriba,* which is 10·621 feet above the sea, and is at the end of the surface of pumice, at the point of intersection of the two branches of lava, between which we had been ascending. Here my guide declined to proceed any higher that evening, because we should not be able to arrive at the top of the Peak before dark; and even if we gained the summit, we should be obliged to remain there all night without the slightest shelter, which would have been fatal at that season of the year. I was therefore obliged to defer my journey till next morning, and to retrace my steps to the *Estancia.*

The remaining hours of light were employed in endeavouring to obtain some shelter from the wind, by forming a wall of lava and pumice: for some time all our various contrivances

vances were useless; as it blew in eddies and frequently scattered our fires: but two hours after sunset the violence of the gale very much abated, and only came in hollow gusts with a noise similar to that of distant thunder in a mountainous country.

My guide and muleteer soon forgot all their fatigues in a peaceful slumber; but the scene around me was so strange and interesting, and my feelings were so closely allied both to pleasure and pain, that sleep was completely banished from my eyes. My imagination took me to that distant period, when the Cañadas upon which the Peak is situated was an inflamed gulf of volcanic matter, twenty miles in circumference, and nearly a thousand feet deep, sending forth on all sides torrents of liquid lava, raising plains into high mountains or sinking elevated lands into valleys, and creating by degrees the celebrated volcano upon which I was placed, and which must have so frequently threatened destruction to the interesting people who were so cruelly exterminated by the sword of the Spaniards. But everything around me was now calm and placid: the valleys and mountains below were hidden from my view by white fleecy clouds, which had the appearance of an immense plain of snow some hundred square miles in extent; and towering above this sea of vapours, like rocks in the ocean, were the elevated lands of Canary, the mountain of Angostura* in the Cumbre, Montaña Blanca † above the Valley of Orotava, Pedrogil ‡ on the S.E. side of the same valley, and the Risco of *Guajara* §, which is a part of the elevated chain of mountains surrounding the Cañadas from E. to W.S.W. From the refractive state of the atmosphere, these elevations appeared to be higher than they actually were. This was particularly marked by the neighbouring mountain of *Guajara*, which is only nineteen feet higher than the *Estancia*; yet at night it appeared to be considerably above it.

The blueness of the zenith was such, that a person who had not witnessed it would have supposed it unnatural if he had seen it represented in a picture; and from the clearness of the atmosphere, the light given by the stars and planets was sufficient to enable me to see to write my observations; and Venus left a faint glimmering of light on a wreath of snow near my resting-place; and when the moon, which was just entering her first quarter, arose, I could distinctly see the degrees

* Which is 7070 feet above the level of the sea.
† Which is 6731 feet above the level of the sea.
‡ Which is 6148 feet above the level of the sea.
§ Which is 9949 feet above the level of the sea.

upon

upon my thermometer. Another still stronger proof of the extreme clearness of the atmosphere is, that I observed the moon to be indented like a saw, between the light and obscure part, which I suppose was caused by the projection of the enlightened tops of her mountains upon the part which was deprived of the sun's light. At first I thought it was some optical illusion, as I had just before been standing before the fire, and was almost blind by the smoke; but repeated observations which I made during the night convinced me that I was right in what I first observed.

There is another observation which I made that may be worth mentioning. Soon after the sun went down the wind became much louder and had an acuter sound, although the force was very considerably less than in the day-time. It has been observed from the earliest antiquity, that the air becomes more sonorous at night than in the day; but I am not aware that the cause of it is well ascertained. The general opinion, I believe, is, that the air becoming colder, is therefore denser and more susceptible of conveying the sonorous waves. This to a certain extent may be correct, as it has been well ascertained by Dr. Priestley, that the force of the pulsations of sound depends considerably upon the degree of density or rarefaction of the air; and I think Captain (now Sir Edward) Parry mentions the surprising distance he was enabled to hear sound during the winter at the North Pole. From frequent observations which I have made in Teneriffe, I am inclined to attribute the intensity of sound at night to a certain increase of moisture, and to an *equability* of temperature in the different strata of the atmosphere. The increased intensity of sound, when I was on the Peak during the night, could not have been caused by an increased density of the atmosphere; because instead of becoming colder, it was four or five degrees warmer when the sound of the wind became more sonorous. Humboldt has made a similar remark; and as my observations fully coincide with his opinion, I beg to quote it. He ascribes the diminution of sound during the day to the presence of the sun, which influences the propagation and intensity of sound, by opposing to them currents of air of different density, and partial undulations of the atmosphere produced by unequal heating of different parts of the ground. In these cases a wave of sound, when it meets two portions of air of different density, is divided into two or more waves, a part of the primitive wave being propagated with more rapidity through the denser portions than the parts that pass through air of less density. In this way the wave is broken down into different parts, which arrive at the ear at different times. These different portions of

of the wave passing again through succeeding portions of the atmosphere of different density, may be so wasted and frittered down as to be incapable of affecting the tympanum.

My observation respecting the intensity of sound is not confined to the Peak. At the town of Orotava, situated about two miles from the sea, the noise of the waves in the morning occasionally had a grave low tone: at the same time the air appeared to be particularly *dry*, and distant objects were very indistinct. Towards the middle of the day, or the beginning of the afternoon, the island of Palma, nearly sixty miles distant, could be distinctly seen; and the ridge of mountains that surround the valley of Orotava were apparently brought so close, that the vegetation upon them could be observed: at the same time the sound of the sea invariably passed from a grave to an acute sound. The natives prognosticate rain when this particular clearness of the atmosphere takes place; and I have generally found them correct.

But to return from my digression. At various times in the night I observed meteors, like rockets, with luminous points, shooting about in the atmosphere, apparently at an elevation not much greater than the top of the Peak. Their appearance was different from those ignited vapours commonly called falling-stars, and their course was different, as they generally moved in a horizontal direction.

At no period of the night did the thermometer fall below 34°, and the average height of it from 5 P.M. to 5 A.M. was 37°; but from the great rarefaction of the air, it appeared by the feelings to be considerably below the freezing point.

[To be continued.]

XXII. *Notices respecting New Books.*

A Treatise on Hydrostatics and Hydrodynamics for the Use of Students in the University. By H. Moseley, *B.A. of St. John's College.* Cambridge, 1830.

THIS work contains a proposition deserving of notice, as being in a great measure new, and of considerable importance in the theory of the motion of fluids. The proposition may be thus generally stated:—If fluid of any kind be moving in such a manner that at the same point in space the velocity is constantly the same in quantity and direction, and if $x, y, z,$ be the forces impressed at any point whose coordinates are $x, y, z,$ and v be the velocity at this point, then will

$$\int \frac{dp}{\varrho} = \int (X\,dx + Y\,dy + Z\,dz) - \frac{v^2}{2} + c$$

Euler has given in the Berlin Memoirs, 1755, a proof of this theorem for incompressible fluids, seemingly without being aware

that

that it may be extended to other fluids. The demonstration of Mr. Moseley, which is as follows, is remarkable for simplicity and is perfectly general. Let ϕ, ϕ', ϕ'', be the effective accelerative forces in the directions of the axes of coordinates, at any point whose coordinates are x, y, z. By D'Alembert's Principle,

$$\int \frac{dp}{\varrho} = \int \left((X - \phi)\,dx + (Y - \varphi')\,dy + (Z - \phi'')\,dz \right),$$

the integration being performed with respect to x, y, z, only; t the time being constant. But when the motion is uniform, a given particle in the *successive instants* of its motion, passes through the states of the particles, which at a *given instant* are situated on the path of its motion. Hence, if the integral above be taken in reference to an arbitrary portion of this path, the term $\int(\varphi\,dx + \varphi'\,dy + \varphi''\,dz)$ may be taken in reference to the motion of a given particle along this portion. But in this case we know from the principles of dynamics that $\int(\varphi\,dx + \varphi'\,dy + \varphi''\,dz) = \frac{v^2}{2} - c$, the constant c depending on the motion at the arbitrary limit. Hence the proposition enunciated manifestly follows.

It may be readily shown from this theorem, that if $p = a^2\varrho$, P = the atmospheric pressure, P′ the pressure in a large vessel of air maintained in a given state of compression, the velocity of issuing from a small orifice into the atmosphere is determined by the equation $v^2 = 2\,a^2$ hyp. log. $\frac{P'}{P}$, and not by $v^2 = 2\,a^2\,\frac{P - P'}{P}$ as is usually supposed. M. Navier has come to the same conclusion, (*Mémoires de l'Academie des Sciences*, tom. ix.) by reasoning, however, upon the hypothesis of parallel sections. Mr. Moseley has not neglected this application of his theorem.

Mr. Moseley's Treatise contains a careful statement of the principles of hydrostatics and hydrodynamics, and detailed solutions of a great variety of problems. There is also a chapter on hydrostatic machines, in which are given an account, and a theory of the action, of Montgolfier's hydraulic ram.

XXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

THE following are titles of papers which have been read before the Royal Society during the latter part of the Session, which ceased in June last.

March 25.—Experiments to determine the difference in the number of vibrations made by an invariable pendulum in the Royal Observatories of Greenwich and Altona. By Captain Sabine, Roy. Art. Sec. R.S.

Experiments to ascertain the correction for variations of temperature, within the limits of the natural temperature of the climate of the South of England, of the invariable pendulum recently employed by British observers. By the same.

April 29.—Researches in physical astronomy. By John William Lubbock, Esq. F.R.S.

June 17.

June 17.—On a new register-pyrometer for measuring the expansions of solids, and determining the higher degrees of temperature upon the common thermometric scale. By J. Frederick Daniell, Esq. F.R.S.

GEOLOGICAL SOCIETY.

May 21.—Grenville Lonsdale, Esq., Ensign in the Third Foot, was elected a Fellow of this Society.

At this Meeting, Messrs. Sedgwick and Murchison's paper on the Austrian Alps was read, which is published in our present Number.

June 4.—Rev. Richard Dawes, M.A., Fellow and Tutor of Downing College, Cambridge; Rev. Charles Currie, M.A. Fellow of Pembroke College, Cambridge; Rev. Thomas Musgrave, M.A. Fellow of Trinity College, Cambridge; William Devonshire Saull, Esq. of Aldersgate Street, London; and Francis Ellis, Esq. of the Royal Crescent, Bath,—were elected Fellows of this Society.

A paper was read, entitled "On the Geological Relations of the South of Ireland, by Thomas Weaver, Esq. F.G.S. F.R.S. M.R.I.A., &c."

This Memoir gives an outline of the mineral constitution of a large tract in the south of Ireland, comprising the counties of Cork, Kerry, and Clare, with part of those of Galway, Tipperary, and Waterford; and thus connecting this portion of the island with the eastern part of it, formerly described by the author.

This hilly and diversified region is chiefly composed of ridges, having generally a direction from east to west, and attaining their greatest elevation in the mountains of Kerry, where Gurrane Tual, one of Magillycuddy's Reeks, near Killarney (the highest land in Ireland), is 3410 feet above the sea.

The rocks in this elevated country are chiefly of the transition class: they decline gradually towards the north, and finally pass under the old red sandstone and carboniferous limestone of the midland counties.

1. Transition Series.

In Kerry there is a persistent series of transition rocks, having a general direction from east to west, and dipping to the north and south with vertical beds in the axes of the ridges: the strata as they diminish in inclination on each side, form a succession of troughs. The principal rock-masses are composed of grauwacke, slate, and limestone; but the general series is distinguished, by the author, into simple and compound rocks; the simple being clay-slate, quartz-rock, hornstone, lydian-stone, and limestone. The compound sandstone and conglomerates with bases of clay-slate, quartz, and sandstone; grauwacke, and grauwacke-slate; sandstone and sandstone-slate; greenstone; and hornstone-porphyry. Roofing-slate, though comparatively rare, is found of an excellent quality in the island of Valentia.

Organic remains occur more frequently in the limestone of this series than in the slate and grauwacke. In Kenmare these remains consist of a few bivalves, and some crinoidal remains; and these also

are most numerous in the Muckruss and Killarney limestones. At the foot of the Slieve-meesh range this limestone includes Asaphus caudatus, Calymene macrophthalma, and perhaps a third crustaceous animal, with Orthoceratites, Ellipsolites ovatus, an Ammonite, Euomphalites, Turbinites, Neritites, Melanites, and several species of Terebratula, Spirifer, and Producta. Other bivalves in this locality are referrible to species figured by Schlotheim, as from transition rocks on the Continent.

Near Smerwick harbour, similar organic remains are abundant in slate, and fine-grained grauwacke, together with Hysterolites, and many genera of polyparia; the whole resembling both in mineral and zoological characters the rocks of Tortworth in Gloucestershire, formerly described by the author, as well as those of the Taunus in Nassau, more recently described by Sir Alexander Crichton. Again, the same fossils are found in the limestone of Cork, associated with impressions of vertebræ of fishes; and analogous remains are to be met with also in a portion of the slate of that neighbourhood.

Transition coal.—All the coal of the province of Munster, except that of the county of Clare, is referrible to one of the earliest periods at which that mineral has been produced; the true coal overlying the mountain limestone being found in that county alone. At Knockasartnet, near Killarney, and on the north of Tralee, thin anthracitic beds, inclined at various angles from 70 degrees to verticality, are included in grauwacke and slate. In the county of Cork this old coal is more extensively developed, particularly near Kanturk, extending from the north of the Blackwater to the Allow. The gorges of the latter river, and various other neighbouring defiles, expose clay-slate, grauwacke, shale, and sandstone, in nearly vertical beds, directed from west to east. This transition tract extends to the river Shannon on the north-west. As the systems range from west to east, in a series of parallel, acutely angled troughs, the beds have great diversity of inclination, dipping rapidly either to north or south, and bending to horizontality between the ridges. This coal or anthracite is raised in sufficient quantities for the purpose of burning the limestone of the adjoining districts; and the most considerable collieries, those of Dromagh, have yielded 25,000 tons per annum, at from 10*s*. to 15*s*. per ton.

The coal, and accompanying pyritiferous strata are abundantly charged with the remains or impressions of plants, belonging chiefly to Equiseta and Calamites, with some indications of Fucoides. Beds of transition coal occur also in the county of Limerick, on the left bank of the Shannon, north of Abbeyfeale, and at Longhill; and are seen, though in very small quantity, on the right bank of the river at Labbasheada. Several other places where coal strata occur, are mentioned by the author.

The transition rocks of Kerry and Limerick are prolonged into Cork and Waterford, preserving with certain modifications an analogous character and composition. The carboniferous limestone reposing upon this tract, on the north, is usually unconformable to it, but is conformable to the old red sandstone, wherever that rock intervenes. In this system of strata, organic remains, such as polyparia,

paria, bivalves, Trilobites, &c. occur near the Bonmahon river; the horizontal planes which they occupy crossing the vertical cleavage of the slaty grauwacke nearly at right angles. The series rests upon, and passes into clay-slate, and is capped by old red sandstone and strata of the carboniferous order. Metalliferous veins with indications of copper and lead are seen in the cliffs of the transition series, east and west of the Bonmahon river.

II. *Metalliferous relations in Kerry and Cork.*

The author having succeeded in restoring the copper mines at Ross Island, on the Lake of Killarney, and in effectually draining off the water, was enabled to prove that the ore did not constitute a metalliferous bed, or any real vein, but was contemporaneous with the rock in which it is irregularly distributed in the form of ribs, branches, strings, &c., analogous to those of calcareous spar, in limestone. The rocks at Ross Island consist of blue limestone, and beneath it of siliceous limestone, but the ore is confined exclusively to the former; and various trials have proved the non-existence of any vein communicating with the metalliferous deposit. Copper ore is similarly distributed at Crow Island:—but at the Muckruss mines the ore was obtained chiefly from a metalliferous bed. The author has ascertained exactly the extent of the limestone bearing lead in Kenmare, where most of the unsuccessful trials in search of ore have shown that the mineral deposits are discontinuous, and nearly parallel to the range and dip of the beds; and in Castlemaine mine, where lead ore was formerly worked in a mass of calcareous spar and quartz, it thinned out into an unproductive pipe. Near Tralee and Ardfort, and on the left bank of the Shannon, lead ore has been unprofitably worked in limestone, sandstone and slate.

In the county of Cork, the copper mines are those of Allihies, Audley, and Ballydehol; and those producing lead are situated at Doneen and Rinabelly. The mine at Allihies is one of the richest mines in Ireland; it was discovered only in 1812, and has already yielded more than 2000 tons of copper ore per annum. The ore occurs in a large quartz-vein, which generally intersects the slaty rocks of the country from north to south, but in some places runs parallel to the stratification. It is remarked that all this portion of the county of Cork indicates a very general diffusion of cupreous particles, so much so, that in the year 1812 there existed a cupriferous peat-bog on the east side of Glandore harbour, forty or fifty tons of the dried peat producing when burnt, one ton of ashes, containing from ten to fifteen per cent of copper. The lead mines of Doneen and Rinabelly are in slate.

In concluding a long series of observations on the mines of the tracts described in this paper, the author remarks that the diffusion of metallic substances throughout the mass of rocks is far from being an uncommon occurrence—the metalliferous matter appearing in isolated particles, and in strings, veins or filaments, more or less connected with each other, but not continuous or persistent, and therefore of contemporaneous origin with the rock itself.

III. *Car-*

III. *Carboniferous series of Clare.*

The clay-slate formation in this county is bordered by a belt of old red sandstone, to which succeed, in ascending order and conformable position, the mountain limestone and coal measures, both of which occupy flat and undulating hills, and the strata usually dip from the east of north to the west of south; but seldom at a greater angle than 5°. The best sections are seen in the cliffs of the west coast, where shale, sandstone and sandy-flag-stone overlie limestone. Coal, however, is there of very rare occurrence, and when disclosed is of very indifferent quality; and the author infers, that the lower part of the series in the county of Clare is comparatively poor in this mineral: he, however, suggests that the best chances of discovering valuable seams must lie in the elevated regions of Mount Cullun; where if coal be found, the beds being nearly horizontal, it might be worked with advantage.

The Memoir concludes with some observations on the distribution of diluvial matter in the South of Ireland.

1. Boulders, gravel and sand, derived from the transition series are lodged along the borders and sides of the mountains in Kerry.

2. In a small district of Limerick and Tipperary, situated between the Gaultees and Slieve-na-muck, the rolled debris consist not only of portions of the contiguous rocks, but contain also porphyry, which is not to be found *in situ* near the vicinity of Pallis Hill.

3. In the peninsula of Renville, near Galway, the surface of the carboniferous limestone is strewed over with numerous boulders of red and gray granite, syenite, greenstone, and sandstone, which must apparently have been conveyed from the opposite side of the bay of Galway.

June 18.—Robert Dawson, Esq. of the Royal Engineers, and employed on the Ordnance Survey of Ireland, was elected a Fellow of this Society.

A letter on the Basin of Alhama, in the Province of Granada, in Spain, being the second of two letters addressed to R. I. Murchison, Esq., Sec. G.S., F.R.S. &c., by Col. Charles Silvertop, F.G.S., was then read*.

The basin of Alhama is situated about 50 miles to the south-west of the basin of Baza, which was described in the former letter. It occupies a large circular area, bounded on the south and east chiefly by the primitive chain of the Sierra Nevada, and on the north-west and south-west by ridges of nummulite-limestone. The greater diameter of the basin, namely, between the village of Huerta de Santillana on the north, and the ridge near Alhama on the south, is about 36 miles; and the smaller diameter, between the village of Escujar on the east, and the town of Loja on the west, is about 30 miles. The principal river traversing the basin is the Genil, which takes its rise in the Sierra Nevada to the east of Granada; and having received all the minor streams which water the basin, it passes through a chasm in the nummulite-limestone near Loja, and afterwards unites with the Guadalquiver.

* For the first letter, see Phil. Mag. and Ann. of Phil., vol. vii. p. 453.

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The whole area of the basin, with the exception of an insulated group of transition limestone rocks near Granada, is occupied by conglomerates, marl, gypsum, and limestones containing freshwater shells. The conglomerates predominate to the north and east of Granada, and form a high tract of waving hilly ground between that city and the eastern part of the Sierra Nevada; and the other deposits prevail through the southern portion of the basin. The valley through which the Genil flows is the lowest part of the district, and is composed near Granada of a disintegrated conglomerate.

The author gives a detailed account of the geological appearances presented along the line of road from Granada to Alhama. The lower strata consist of beds of gypsum alternating with strata of marl and marly, micaceous sandstone. The gypsum is in general of the ordinary, fibrous variety; but near the village of Escuzar, alabaster of a beautiful whiteness is quarried. In the bed of a rivulet passing by La Mala a brine-spring issues, which yields from 18,000 to 24,000 fanegas of salt yearly; the fanega being equal to 25lbs Spanish. The strata of marl and gypsum are covered with a compact limestone, containing casts of Paludinæ; and on this limestone rest irregular masses composed almost entirely of comminuted shells of the genera Limnæa and Planorbis. The fossils found in these limestones have been examined by Mr. J. Sowerby, who has supplied the following list:—

Planorbis rotundatus, found in the Isle of Wight.
Planorbis rotundatus vel planulatus.
Planorbis, new species.
Bulimus pusillus, of Broard.
Paludina pusilla, of Deshayes.
Paludina Desmarestii.
Paludina pyramidalis.
Ancylus.
Cypris.
Limnæa.

The structure of the country around Alhama is explained by three sections in the immediate vicinity of that village. One of these, observed by following the horse-road from Alhama towards Loja, presents in an ascending order the following succession of horizontal strata, and may be taken as the type of the others.

1. The nummulite limestone, which constitutes the boundary of part of the basin.

2. A coralline limestone, which in some parts alternates with a calcareous sandstone and a fine-grained conglomerate; the sandstone abounds with a Pecten, which resembles the Pecten reconditus of the London clay.

3. The rock composed of alternate strata of gypsum and marl.

4. The freshwater limestone with Paludina, above described, which forms a table land, extending in the direction of Loja as far as the eye can reach.

Under the freshwater limestone, near the village of Arenas, is a large deposit of brown coal of unknown depth. The remains of Planorbis are abundant in the upper layers of it.

In conclusion, the author states that he had observed a compact limestone containing Limnæa and Planorbis, near Partaloba, in the province of Granada; Montesa, in the province of Valencia and La Gineta; and Ocaña, in the province of La Mancha;—that he had likewise

wise ascertained the existence of an extensive lacustrine basin near the town of Terruel in the province of Arragon, composed of a coarse limestone containing Limnæa pyramidalis (a fossil of the Isle of Wight), resting upon gypsum and marl.

At the close of this Meeting, which terminated the Session, the Society adjourned till Wednesday Evening, the 3rd of November.

HORTICULTURAL SOCIETY.

May 4.—The following paper was read:—An account of an œconomical method of obtaining very early crops of new potatoes. By Thomas Andrew Knight, Esq. F.R.S. &c. President.

The following specimens, &c. were exhibited:—Sweeney nonpareils, from T. N. Parker, Esq.—One hundred sorts of apples, from Mr. Hugh Ronalds.—Models of apples, pears, plums, cherries, &c. by Mr. William Tuson.—Several sorts of tulips, from Mr. Henry Groom.—Twelve sorts of apples and a collection of flowers, from the Garden of the Society.

The following candidate was balloted for and duly elected:—James Dunlop, Esq.

May 18.—The following papers were read:—Upon the cultivation of Epiphytes of the Orchis tribe. By John Lindley, Esq. F.R.S. &c. Assistant Secretary.—An account of the method of obtaining very early crops of green-peas. By Thomas Andrew Knight, Esq. F.R.S. &c. President.

Exhibited—A dish of forced cherries, from Mr. Benjamin Law. A forced cherry-tree, from the same. A bundle of asparagus consisting of 125 heads, weighing twenty-eight pounds, from Mr. Wm. Robert Grayson, of Mortlake. A scarlet Brazilian pine-apple, from the Garden of the Society. Asparagus blanched in tubes, and also grown in the common way, from the Garden of the Society. A large collection of flowers, from the same place.

Major Gen. Thomas Bligh, St. George, was balloted for and duly elected a Fellow.

June 1.—The following papers were read:—Some account of a new cherry called "the early purple guigne." By Mr. Robert Thompson, under-gardener in the fruit department of the Garden of the Society. —Some remarks upon the cultivation of the strawberry: in a letter to Mr. Lindley. By Mr. John Fairbairn, F.H.S.—On a method of forcing cherry-trees: in a letter to Mr. Lindley. By Mr. Benjamin Law, of Northampton.

Exhibited—Seven sorts of pelargoniums, from Mr. Russell of Battersea. A specimen of a hybrid cactus from the Comte de Vandes. Various flowers from the Society's Garden; together with a Trinidad pine-apple; and specimens of cherries grown under different circumstances.

June 15.—Exhibited—Seedling Azaleas, from the Earl of Carnarvon. Double Sempervivens rose and la Tourterelle rose, from Mr. James Young. Caprifolium pubescens, from Robert Barclay, Esq. Cypripedium spectabile, from Mr. Wm. Malcolm. Cactus speciosissimus,

simus, from Mr. Henry Groom. A collection of pinks, from Mr. T. Hogg. A model of a wheel water-engine, from Mr. Siebe, the inventor. A large collection of flowers, from the Garden of the Society.

The Chairman announced that the Council, having ascertained that there was no part of the Charter or Bye Laws which rendered Ladies inadmissible as Fellows of the Society, had Resolved, in compliance with the wish of various members of the Society, that such Ladies as were desirous of becoming Fellows should be proposed at this present meeting.

A certificate was read in favour of the Countess of Radnor, who being privileged by the Bye Laws was balloted for and duly elected. The following candidates were also balloted for and elected: Joseph Strutt, Esq., and Robert Throckmorton, Esq.

In pursuance of a Resolution of the Council, a proposal for the repeal of the present Bye Laws and a draught of amended Bye Laws was read; and having been signed by the Chairman, was suspended in the Meeting-room.

XXIV. *Intelligence and Miscellaneous Articles.*

NOTES ON DR. ROGET'S REPLY* TO MR. BABBAGE.

THE Secretary of the Royal Society having in his letter to the President attempted to explain away the charges I had brought against the mode of keeping our minutes, proceeds in his "Observations" to assert that they are "*groundless accusations;*" and with singular infelicity himself supplies the most undoubted evidence of their truth.

There are three points on which he complains.

1st. I have asserted that a certain minute of 26th Nov. 1829 is not correctly entered.

To refute this the Secretary states that he *destroyed* the *original minute* ["the rough draft was destroyed . . . ."], and caused to be entered on our minutes a resolution which he himself admits to be at variance with the fact †.

2nd. I have complained of great delay in entering the minutes of the Council in the proper book.

To this Dr. Roget replies, that one ‡ of the three cases I have given (and he must be aware I could easily have enumerated many more) is

* Published in the last Number of the Phil. Mag. and Annals of Philosophy.

† "At the meeting in question a rough minute was of course taken down, "and it *did* contain the name of Captain Beaufort." "That minute was after- "wards *corrected.*"—*Dr. Roget's Observations in reply to Mr. Babbage.*

‡ I shall not follow the Secretary into the special pleading, by which it is denied that the meeting of the 11th of February was a Council. I may perhaps offer a few observations upon it in a subsequent edition of my work; but I think it would be ungenerous to a valuable officer of our Society, who cannot avail himself of the same means of rectifying a misapprehension, to allow it to be inferred, which it might, perhaps, from Dr. Roget's Observa-

 tions,

not an instance of delay. Of what avail is the remark, even were it correct? For the Secretary himself admits that he *purposely* delays them; and he defends that practice lest "*an improper use might be made of them.*" I see no other interpretation of this than to suppose that the Secretary presumes the Fellows of the Royal Society will make an *improper use* of their own documents.

Such are the charges which the Secretary of the Royal Society has pronounced to be "*groundless,*"—a term about as appropriate as that by which in his letter he designates the transition from fact to fiction as ——— *a correction* ["That minute was afterwards CORRECTED"].

3rd. The Secretary having stated in his letter to the President, that I have drawn the "*sweeping conclusion that the* WHOLE *of the minutes* "*are unworthy of the least confidence, and can never hereafter be appealed* "*to as authentic documents,*" ventures in his Observations to point to the concluding paragraph of page 65 of the "Decline of Science in England" for the proof.

I admire the boldness rather than the discretion, or the candour of such a reference. Whoever will take the trouble to turn to that passage, will find that my observation was confined entirely to *one single* Resolution.

To conclude: I have bestowed too much pains on the subject to have any misgivings about the accuracy of the statements I have made respecting the mismanagement of the Royal Society; and if I thought additional evidence necessary for their support, I would invite the Secretary to refute them.

Dorset Street, Manchester Square, 9th July, 1830. CHARLES BABBAGE.

DR. READE'S LECTURES ON VISION.

Dr. Reade, of Cork, requests us to announce that he is about to deliver, at the Mechanics' Institution, a course of lectures on his New Theory of Vision, "demonstrating, from numerous experiments, that the Cornea is the true seat of vision, and that we see by means of erect and reflected, and not by refracted and inverted images."

Dr. Reade also has ready for the press a treatise on the same subject.

REDUCTION OF NITRATE OF SILVER.

In 1826, M. C. de Filière had prepared for him, by one of his pupils, a considerable quantity of nitrate of silver. The finest crystals were put upon blotting paper, and were set aside, out of the contact of any substances floating in the air.

The packet having been examined at the beginning of November last, the paper had assumed a deep violet colour; and he was surprised to find that the crystals, without losing their form, were become perfectly malleable metallic silver.—*Ann. de Chim.* Nov. 1829.

tions, that the Assistant Secretary summoned a Council by "*accident.*" He summoned it by design, according to the regular practice, which he had always followed without censure, and in which he had been instructed by his predecessor in office.

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MAGNETIZING POWER OF THE SOLAR RAYS.

MM. Riess and Moser, after alluding to the doubts which many philosophers entertained as to the accuracy of M. Morichini's experiments, as to the magnetizing power of the solar rays, observe that the favourable results which Mrs. Sommervile obtained, had dissipated the doubts of many persons, and consequently that the supposed discovery had given rise to various theories on the magnetism of the earth and its variations.

The authors then detail the results of their own experiments, which seem to have been made with great care and under varied circumstances: the conclusion at which they arrive, and which seems certainly warranted by their experiments, is, that they have a just claim to reject totally a discovery, which, as they say, has disturbed science at various times during seventeen years. The slight variations which they observed in some of their experiments, and which they have not concealed, cannot, they conceive, arise from a real action of the nature of that described by MM. Morichini and Baumgartner as being so evident and decided; added to which, these variations are not always favourable to the supposed discovery.—*Ann. de Chim.* Nov. 1829.

CONSTITUTION OF ACETIC ÆTHER.

By a series of experimental researches, M. Planiava has arrived at the conclusion, that acetic æther is formed of one equivalent of acetic acid and two equivalents of alcohol; and that therefore it is a sub-acetate of alcohol, and is represented by the number 97.—*Kastner's Archives, Royal Institution Journal.*

PERCUSSION FIRE-ARMS.

The following article, from the Journal of the Franklin Institute, vol. iv., relates to a paper which was published, from the German, in the Phil. Mag. vol. lxvi. p. 197.

Remarks on an article in the Journal of the Franklin Institute for February last, on Fulminating Powders, and their use in Fire-arms. By Joshua Shaw, Esq.

To the Editor.

Sir,—I am induced, from reading an article in the Journal of the Franklin Institute, on the subject of certain fulminating powders written by Lieut. P. Schmidt, of the Prussian service, to send you some remarks, which are the result of much experience upon the point in question. I hope, however, that you will not expect from an operative artist, anything which is very systematic or scientific, for in this case you will be disappointed, as I am equally far from possessing either the ability or the inclination to furnish it. To me, and to many more practical men, the learning which writers appear anxious to evince, seems to predominate over everything else, and thus to destroy the utility of their labours. It is in vain to attempt to give instruction, excepting a language be used with which the pupil is in some degree familiar.

In the paper to which I have alluded, Lieut. Schmidt, in speaking of the

the powder made from oxymuriate of potash, sulphur, and charcoal, observes, that it is made by adding together twelve parts of sulphur, ten of charcoal, and one hundred of the oxymuriate, and then proceeds to give the result of his experiments. My object at present is, not to treat of the best mode of preparing the different fulminating powders, but rather to correct the false statements which are made respecting the utility of the different kinds. I will observe, however, that from long experience, I can aver that the proportions above given are not such as will produce the strongest powder from these materials.

It is perfectly clear to me, that at the time the Lieutenant wrote, the subject was new to him, and, indeed, he speaks of the recent use of the copper caps in Germany. I have been in the habit of using copper caps for at least thirteen years, and for the last seven years have manufactured and sold them, at the rate of two millions annually. After speaking of various contrivances, he says, "Besides these, other devices have been used for the purpose of igniting this kind of powder, yet they have all their defects, and offer so many difficulties in practice, as to have prevented their general introduction." It would then appear that we are, in this respect, much in advance of Germany, as we have had the prepared caps in almost universal use, for many years, and have never met with the difficulties complained of; this may have arisen, in a great degree, from our having fewer prejudices to contend with than the inhabitants of that country.

Lieut. Schmidt alludes to the observations of Mr. Wright, of London, on the use of fulminating mercury, mentions his mode of filling the caps with this material, by means of an ivory rod, to which he objects as being both laborious and dangerous; he then recommends a plan of his own, as being much more safe and expeditious. Although this may be a subject of serious discussion between England and Germany, it must here create a smile only. The Lieutenant speaks of filling several thousands in *a week*; by means of an apparatus which I have invented, and long had in use, a little girl fills several thousand in the course of a few hours, every morning. In about four seconds, 500 of the caps are collected together and arranged for filling, and in about the same time an equal quantity of the powder is deposited in each; it is then, with great rapidity, secured by a cement which renders it impervious to water.

It is said that difficulties have been experienced in England, in igniting gunpowder, by the fulminating mercury, and it appears that the labours of the German professors upon this subject, have resulted in their drawing conclusions altogether erroneous respecting the kind of fulminating powder which should be used in fire-arms. Fulminating silver may be considered as out of the question, on account of its price, and need not, therefore, be further noticed. With respect to that prepared with the oxymuriate, its destructive effects are such as to forbid its use entirely. It soon rusts the lock, penetrates the pores of the iron, rendering it carious, and, consequently, liable to burst. We have long since abandoned the use of it altogether, and are not likely to resume it, in deference either to English or German opinions.

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It is plain that Lieut. Schmidt mistakes the meaning of the word *effect*, as used by Mr. Wright. The fulminating quicksilver makes a louder report than the ordinary fulminating powder, but will not fire the magazine at so great a distance. Its fire is rapid, but not elastic, or expansive, as it quickly condenses, and returns to the state of mercury: it is, however, certain, and presents no difficulties whatever.

The most extraordinary part of the statement is, that the fulminating quicksilver is more corrosive than the oxymuriate preparation! With what difficulty do we sometimes arrive at the most simple truths! Nothing can be more gratuitous and false than the above conclusion. Mr. Forsyth of England expended, it is said, a hundred thousand pounds in his attempts to establish the use of the percussion magazine lock, in which he failed altogether, from the corrosive effects of the powder; the whole 14 years of his patent were devoted to this point. As soon as this period had terminated, Mr. Wright introduced the fulminating mercury, since which there has been no complaint whatever of the corrosion of locks and barrels, excepting from the use of imported caps charged with the old materials.

The cement used is a point of much importance: gum benjamin and gum arabic have been principally employed; the first is always soft, the latter attracts moisture; neither of them answers well. The French, to obviate the defects of both these, sought the remedy by enlarging the caps at the bottom, so that when the powder is introduced and dry, it is, as it were, dove-tailed in, and a smaller quantity of either of the foregoing ingredients will retain it. This lessened, but did not remove the evil.

In America, the percussion gun has, in consequence of the manner in which the caps have been made here, been more generally employed than in England, although the guns themselves are the manufacture of that country. We have, it is true, ran counter to the rules established by the German officer and professors; for, although we may be less scientific in these matters, we know enough of the amusements of the field, to derive from our experience that information which suits us better than the most learned theories, and we even venture to adopt the suggestions of the former, although opposed by the deductions from the latter.

Should you think proper, sir, to give this a place in your Journal, I shall again request a small place for some future observations, containing the practical results of my own experience, as it may interest a certain portion of your readers. Yours, &c.

Philadelphia, March 20, 1829. JOSHUA SHAW.

[We have not met with any further communication on the subject in the Journal of the Franklin Institute.—EDIT. PHIL. MAG.]

METEOROLOGICAL OBSERVATIONS FOR JUNE 1830.

Gosport:—Numerical Results for the Month.

Barom. Max. 30·15. June 2. Wind W.—Min. 29·38. June 22. Wind N.E.
Range of the mercury 0·77.

Mean

Mean barometrical pressure for the month .... 29·860
Spaces described by the rising and falling of the mercury............ 4·590
Greatest variation in 24 hours 0·510.—Number of changes 24.
Therm. Max. 72°. June 25. Wind E.—Min. 45°. June 4. Wind W.
Range 27°.—Mean temp. of exter. air 57°·88. For 31 days with ☉ in Π 56·72
Max. var. in 24 hours 19°·00.—Mean temp. of spring-water at 8 A.M. 49·80

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the atmosphere, in the morning of the 3rd ... 96°
Greatest dryness of the atmosphere, in the afternoon of the 19th 50
Range of the index 46
Mean at 2 P.M. 65°·0.—Mean at 8 A.M. 71°·9.—Mean at 8 P.M. 77·2
—— of three observations each day at 8, 2, and 8 o'clock .... 71·3
Evaporation for the month 3·40 inches.
Rain in the pluviameter near the ground 2·63 inches.
Prevailing winds, S.W. and W.

Summary of the Weather.

A clear sky, 3; fine, with various modifications of clouds, 13; an overcast sky without rain, 8½; rain, 5½.—Total 30 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
24	13	30	0	24	25	19

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
4½	1	1½	5	½	6½	7	4	30

General Observations.—The weather this month has been showery, with the exception of about ten dry days at short intervals; and the frequent, but not heavy rains here have been generally accompanied with cold gales of wind: so that from the comparatively weak sunshine, and humidity of the atmosphere, the growth and ripening of most of the vegetables and fruits, that are looked for at Midsummer, have been retarded. The various kinds of corn, though backward for the season, look remarkably strong, with a promising appearance for plenty. The unexpected continuance of wet and ungenial weather for the last five weeks, it is hoped, will cause a more favourable time for the operations of the ensuing harvest.

Hay-making partially commenced here and in the neighbourhood about the 20th instant; but from a series of showers to the 28th, there was no good opportunity of getting it in: therefore those who deferred cutting their grass till the end of the month will have the best hay; as a long exposure to showery weather very much reduces its saccharine quality, and is often the cause of its ignition in the rick. The crops of grass are generally abundant from the nature of the weather, which appears to be getting more settled for a few days.

The mean temperature of the external air this month is nearly 3½ degrees under the mean of June for the last fourteen years, and it is also about a degree under the mean of May in 1822 and 1828!

From 5 o'clock till nearly sunset in the afternoon of the 2nd, two fine-coloured *parhelia* appeared, one on each side of, and each 22½ degrees distant from the sun's centre: they alternately appeared circular and elongated, and each displayed a white vaporous train sixteen degrees in length, like the tail of a comet reversed, and it terminated evanescently, or without defining its conical point. No trace of a solar halo accompanied these phænomena, but a moist wind and vapours of the *cirrostratus* kind were flowing in from the westward; and their trains were formed by the solar rays passing through the increasing vapour that surrounded them.

In

In the evening two *paraselenæ* also appeared from nine till after ten o'clock, without the bounding edge of a large lunar halo; their distance from the moon's centre was 24½ degrees, each being about two degrees from the interior, and three quarters of a degree from the exterior edge of the halo: they exhibited a faint red tinge, underwent the same changes of figure as the *parhelia* in the afternoon, and had white vaporous trains ten degrees long. These were the best defined *paraselenæ* that we have seen for many years past.

The afternoon of the 25th was sultry, when thunder clouds passed over from the S.E., and very vivid sheet and forked lightning emanated from the clouds in every quarter from sunset till after midnight, which terminated in light rain here, but heavy in other places, particularly in Bath and its neighbourhood, where the rain is said to have come down in such torrents, that it raised the Avon to an overflow in two hours, and for a time gave the streets the appearance of rivers of water. The vivid and momentary flashes of lightning and loud peals of thunder are described as having been awfully grand, and the storm tremendous and appalling from 8 till nearly 10 o'clock. By this storm great damage has been sustained by the heavy rains, and consequently floods, which spoiled and carried off much of the out-lying hay in different counties; and several lives were lost by means of the electric fluid. It appears to have taken its course through Hampshire, Wiltshire, Somersetshire, Gloucestershire, Coventry, Staffordshire, Derbyshire, &c., and it extended to the neighbourhood of London.

The atmospheric and meteoric phænomena that have come within our observations this month, are three parhelia, two paraselenæ, one solar and two lunar halos; and ten gales of wind, namely, two from the North, three from the South-west, three from the West, and two from the North-west.

REMARKS.

London.—June 1, 2. Fine. 3. Fine in the morning: heavy rain. 4. Cloudy; strong wind. 5, 6. Fine. 7. Rainy. 8. Cloudy. 9. Stormy and wet. 10. Cold, cloudy, with frequent showers. 11. Rain in the morning; cloudy. 12, 13. Showery, with some hail. 14. Heavy showers. 15. Fine; rain at night. 16, 17. Cloudy; rain at nights. 18—20. Fine. 21. Fine in the morning: rain. 22. Rain in the morning: clear and cold at night. 23, 24. Very fine and warm. 25. Rainy in the morning: cloudy: thunder at night. 26. Fine. 27—29. Fine, with brisk wind. 30. Very fine.

Penzance.—June 1. Clear. 2. Fair: misty rain. 3. Rain. 4. Clear. 5. Clear: a shower. 6. Fair: rain. 7—10. Clear. 11. Fair. 12. Rain: fair. 13, 14. Fair: clear. 15. Clear: a shower. 16. Showers: fair. 17. Clear. 18. Fair. 19. Clear. 20. Fair: rain. 21. Rain. 22. Fair: clear. 23. Fair. 24. Rain. 25. Misty rain. 26. Fair. 27. Fair: rain. 28. Fair: showers. 29. Clear. 30. Fair.

Boston.—June 1. Cloudy. 2. Fine. 3. Cloudy: rain P.M. 4. Cloudy. 5. Fine. 6. Fine: rain P.M. 7. Rain. 8. Cloudy. 9, 10. Rain. 11. Fine. 12. Fine: rain early A.M. 13. Cloudy. 14. Cloudy: showers during the day, with thunder and lightning. 15. Cloudy: rain A.M. and again at night. 16. Cloudy: rain at night. 17, 18. Cloudy. 19. Cloudy: rain early A.M.: rain and hail with tremendous thunder and lightning 1 P.M. 20, 21. Cloudy. 22. Cloudy: rain P.M. 23, 24. Fine. 25. Cloudy. 26. Cloudy: rain with thunder and lightning early A.M. 27. Fine: rain P.M. with thunder and lightning. 28. Cloudy: rain P.M. 29, 30. Fine.

Meteoro-

Meteorological Observations made by Mr. Booth at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Giddy at Penzance, Dr. Burney at Gosport, and Mr. Veall at Boston.

Days of Month, 1830.	Barometer. London. Max.	Barometer. London. Min.	Barometer. Penzance. Max.	Barometer. Penzance. Min.	Barometer. Gosport. Max.	Barometer. Gosport. Min.	Barometer. Boston 8½ A.M.	Thermometer. London. Max.	Thermometer. London. Min.	Thermometer. Penzance. Max.	Thermometer. Penzance. Min.	Thermometer. Gosport. Max.	Thermometer. Gosport. Min.	Thermometer. Bost. 8½ A.M.	Wind. Lond.	Wind. Penz.	Wind. Gosp.	Wind. Bost.	Evap. Gosp.	Rain. Lond.	Rain. Penz.	Rain. Gosp.	Rain. Bost.
June 1	30·087	30·037	30·00	30·00	30·12	30·10	29·35	65	46	60	53	63	51	53	SW.	W.	SW.	W.	...	...	...	...	...
2	30·150	30·037	30·10	29·95	30·15	30·07	29·53	70	50	61	51	66	50	58	SW.	S.	SE.	W.	...	...	...	0·100	...
3	29·789	29·585	29·75	29·65	29·77	29·60	29·34	62	50	56	52	61	51	62	SW.	NW.	SW.	E.	0·30	0·91	1·000	·680	...
4	29·937	29·734	29·75	29·70	29·92	29·67	28·90	62	41	58	49	61	45	56·5	SW.	N.	W.	W.	...	·02	...	...	0·64
5	30·020	29·949	30·00	29·90	30·03	30·01	29·40	72	49	63	45	64	49	57	SW.	S.	SE.	W.	...	...	0·045	...	...
○ 6	30·008	29·987	30·00	30·00	30·05	30·03	29·40	72	54	62	52	65	52	57	SW.	NW.	W.	W.	·40	·13	·330	·145	...
7	29·949	29·876	30·04	30·00	29·98	29·91	29·25	63	50	60	52	64	56	59·5	NW.	NW.	NW.	W.	...	·05	...	·095	·11
8	30·070	30·040	30·10	30·10	30·08	30·04	29·47	60	49	56	48	63	50	53·5	N.	NE.	N.	NW.	...	·04	...	·070	·27
9	30·076	30·034	30·08	30·08	30·07	30·05	29·50	58	49	60	49	57	50	53	NW.	NE.	N.	E.	·30	·03	...	·030	·07
10	30·018	30·000	30·10	30·05	30·05	30·03	29·40	58	46	63	49	65	52	53	NW.	N.	NW.	NW.	...	·05	...	·040	·05
11	29·987	29·936	30·00	29·90	30·03	29·94	29·37	68	54	62	52	67	56	58·5	SW.	W.	W.	W.	...	·18	...	·175	·32
12	29·738	29·724	29·90	29·85	29·85	29·80	29·12	59	55	61	52	66	49	59	SW.	W.	W.	W.	·30	·01	·190	...	·19
☾ 13	29·791	29·692	30·00	30·00	29·85	29·80	29·15	55	50	62	51	60	50	57·5	W.	N.	NW.	W.	...	·01	...	·080	...
14	29·706	29·588	30·00	29·95	29·78	29·67	29·06	63	46	61	50	63	48	53	NW.	N.	NW.	NW.	...	·20	...	...	...
15	29·710	29·666	29·90	29·90	29·74	29·72	29·13	63	47	58	50	60	50	53·5	NE.	N.	NW.	NW.	·40	·30	...	·240	·53
16	29·876	29·646	29·85	29·82	29·86	29·64	29·20	61	46	56	50	61	46	54	NE.	NE.	N.	E.	...	·03	·050	·140	·30
17	29·943	29·935	29·95	29·90	29·96	29·94	29·37	58	45	58	51	58	46	53	NE.	NE.	N.	NE.	...	·04	...	...	·06
18	29·846	29·655	29·90	29·80	29·89	29·72	29·21	66	48	61	50	64	52	57	W.	NW.	SW.	W.	·30	·09	...	·010	...
19	29·771	29·643	29·80	29·75	29·75	29·71	29·10	57	49	61	51	63	50	50·5	W.	NW.	W.	NW.	...	...	...	...	·18
● 20	29·637	29·614	29·65	29·60	29·70	29·65	29·00	68	47	61	52	64	47	52	W.	SE.	W.	W.	...	...	...	...	·19
21	29·526	29·481	29·60	29·30	29·56	29·44	29·01	64	48	58	54	66	49	54·5	E.	SE.	SE.	E.	·50	·19	·530	·310	...
22	29·711	29·415	29·60	29·32	29·70	29·38	28·94	65	39	59	54	59	48	54	W.	E.	NE.	NE.	...	...	...	...	...
23	29·886	29·827	29·70	29·70	29·91	29·88	29·31	68	50	64	47	63	54	55	SE.	SE.	SE.	NW.	...	...	...	·030	·04
24	29·887	29·846	29·70	29·65	29·90	29·85	29·35	70	55	58	54	68	57	61·5	E.	SE.	E.	SE.	·25	·23	·650	·280	...
25	29·764	29·596	29·55	29·55	29·76	29·59	29·22	69	58	63	56	72	60	60	E.	S.	E.	E.	...	·09	·080	·080	...
26	29·799	29·691	29·60	29·55	29·83	29·68	29·05	76	54	67	54	71	56	62	W.	SW.	SW.	SE.	...	...	...	...	·45
27	29·867	29·796	29·70	29·70	29·87	29·83	29·20	81	56	67	56	71	57	67	SW.	W.	S.	calm	·20	·02	·215	·040	...
☽ 28	29·863	29·802	29·70	29·68	29·86	29·82	29·18	67	56	66	56	72	56	64	SW.	SW.	SW.	S.	...	...	·135	·085	·43
29	29·947	29·865	29·90	29·85	30·01	29·95	29·20	73	46	65	52	67	51	63·5	SW.	W.	SW.	W.	...	...	...	...	·12
30	30·024	30·014	29·90	29·90	30·06	30·02	29·46	72	52	65	50	65	56	61·5	E.	S.	SE.	NW.	·45	...	...	...	...
	30.150	29·415	30·10	29·30	30·15	29·38	29·24	81	39	67	45	72	45	57·1					3·40	2·62	3·225	2·630	3·95

THE

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[NEW SERIES.]

SEPTEMBER 1830.

XXV. *Table of the Atomic Weights of Simple Bodies, according to* Thomson *and* Berzelius, *with a mean Weight deduced, for each Substance. By Mr.* JOHN PRIDEAUX.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

THE well-earned high reputation of Dr. Thomson, who has, perhaps, done more for the diffusion of the science than any contemporary chemist; the simplicity of the experiments on which his "First Principles" were founded; and the mathematical precision of his results;—gave a general currency to his numbers, the more highly appreciated from their conciseness and facility. And it is proportionately disappointing, when habituated to their convenient application, to observe, that, of several of those experiments which have been disputed, hardly one, in other hands, has exactly corresponded to his results.

If thus obliged, for the present, to defer our expectation of precise knowledge of the relative atomic weights, we are, however, not required to undervalue the importance of Dr. Thomson's investigations. It is a practice, in atomic inquiries, to obtain approximations by different modes of operation, and take a mean number, subject to such corrections as may seem requisite.

We have now two general tables of atomic weights, obtained by different experimental methods, and on the authority of men of the first eminence in the science of Chemistry. Although considerably at variance, yet each is so near the truth as to have been adopted by a great body of the most competent judges; the numbers of Berzelius being generally used on the continent of Europe, and those of Thomson in Britain and America. Is it not probable, until more definitive

tive methods shall be known, that a regulated mean would be a nearer approximation than either?

The following Table for correcting the scale, mentioned in my last communication*, is constructed upon this principle. The experiments which led me to it being comparatively few, it would not have been offered to your pages but for the obvious reasons above quoted. For such a purpose three or four places of decimals would have been inadmissible; nor does any one employ a scale of equivalents for delicate proportions, unless as a banker uses an interest-table to check his calculations.

Table of Atomic Weights of Simple Bodies, according to Thomson *and* Berzelius; *with a mean weight deduced.*

	Thomson.	Berzelius.	Mean.		Thomson.	Berzelius.	Mean.
Aluminum	1·25	171·167	1·2 (*a*)	Iron	3·5	339·213	3·45
Antimony	5·5	806·452	5·5 (*b*)	Lead	13·	1294·498	13·
Arsenic	4·75	470·042	4·73	Lithium	1·25	127·757	1·26
Azote	1·75	177·036/2	1·76	Magnesium	1·5	158·353	1·55
Barium	8·75	856·88	8·66	Manganese	3·5	355·785	3·53
Bismuth	9·	1330·376	9· (*c*)	Mercury	25·	1265·82	12·55 (*k*)
Boron	1·	135·983	0·95 (*d*)	Molybden[m]	6·	598·525	6·
Bromine	10·†	978·3/2	9·9	Nickel	3·25	369·675	3·4 (*l*)
Cadmium	7·	696·767	7·	Osmium‡		1244·21	12·53
Calcium	2·5	256·019	2·53	Oxygen	1·	100·00	1·
Carbon	0·75	76·437	0·76	Palladium‡	7·	665·84	6·7
Cerium	6·25	574·718	6·	Phosphorus	1·5	196·155	1·96 (*m*)
Chlorine	4·5	442·65/2	4·46 (*e*)	Platinum‡	12·	1233·26	12·42
Chromium	3·5	351·819	3·51	Potassium	5·	489·916	4·95
Cobalt	3·25	368·991	3·4 (*f*)	Rhodium	5·5	651·4	6·56 (*n*)
Columbium	18·	1153·715	18· (*g*)	Selenium	5·	494·582	4·98
Copper	4·	395·695	3·98	Silicon	1·	277·478	1· (*o*)
Fluorine	2·25	233·8/2	2·3	Silver	13·75	1351·6	13·63
Fluoron	0·25		0·3 (*h*)	Sodium	3·	290·897	2·96
Glucinum	2·25	331·479	2·22 (*i*)	Strontium	5·5	547·285	5·49
Gold	25·	1243·013	25·	Sulphur	2·	201·165	2·
Hydrogen	0·125	12·4796/2	0·125	Tellurium	4·	806·452	4·
Iodine	15·5 / ·16†	1578·29/2	15·8	Thorinum	‖	744·9	7·45 (*p*)
Iridium‡	3·75	1233·26	12·42	Tin	7·25	735·294	7·3
				Titanium	4·	389·092	3·95
				Tungsten	15·75	1183·2	14· (*q*)
				Uranium	26·	2711·36	26·5
				Yttrium	4·25	401·840	4·14 (*r*)
				Zinc	4·25	403·226	4·2 (*s*)
				Zirconium	5·	420·238	4·6 (*r*)

* See Phil. Mag. and Annals, N.S. vol. vii. p. 276.

† Not given by Thomson, but calculated from Berzelius's last experiments by Thomson's number for Chlorine.

‡ See the note on Rhodium. ‖ Quarterly Journal, April 1830.

(*a*.) Alumi-

(*a.*) Aluminum.—The number of Berzelius for this metal is influenced by his Canon, of the proportions observed by negative substances in combinations; which makes 2 atoms of aluminum combine with 3 of oxygen ($\dot{A}l^{2}$ *) to form alumina. In following the simpler views of British chemists, as better suited to the sliding scale, no inference is intended hostile to that Canon; which has perhaps not been justly appreciated in this country generally†. Berzelius deduces his number from an experiment (*Essai sur le Théorie des Proportions chimiques*, p. 148), where in 100 sulphate of alumina gave, by a red heat, 29·934 of alumina. Neglecting the Canon, this will make alumina $\frac{29{\cdot}9}{70{\cdot}1} \times 5 = 2{\cdot}14$; the mean, with 2·25, Thomson's number, $= 2{\cdot}2$, and hence aluminum 1·2.

(*b.*) Antimony.—The experiments of Thomson giving very direct results, confirmed by Phillips's analysis of tartar emetic (Ann. Phil. 2nd Ser. ix. 373), his number is retained; although the statements of Berzelius (*Essai*, p. 123), and more at large Ann. Phil. iii. 248, throw doubts upon its accuracy.

(*c.*) Bismuth.—The experimental reasons for the alteration of the number for bismuth in Berzelius's last table (*Ann. Ch.* xxxviii. 427) not being given, his construction of the oxide only, $\dot{B}s^{2}$, can be referred to for the explanation. Hence two-thirds of his number $= 8{\cdot}869$ must be taken to compare with Thomson's; when they approached so nearly, that under the circumstances no change seemed desirable.

(*d.*) Boron.—According to Berzelius's former views (*Essai*, p. 126), the atom of boron was 69·655: the experiments on which the change is founded are not given. It is probable they are of importance, as they have led him to such a curious construction of boracic acid as $\dot{B}o^{2}\,\dot{O}^{6}$; but until the 4th volume of his French edition of Chemistry comes out, in which they will probably appear, we can hardly reckon upon them. To compare his new number with that of Thomson, we must take two-thirds of it $= 93{\cdot}988$.

Thomson makes fluoboric acid $= 4{\cdot}25$; and fluoric $= 1{\cdot}25$ (First Prin. i. 161); but if fluoric acid be taken at a mean of their numbers 1·3, boracic acid will turn out $4{\cdot}25 - 1{\cdot}3 = 2{\cdot}95$

* The exponent ^{2} is substituted for the bisecting line, to facilitate the printing; as the latter would require type on purpose.

† It seems to have been regarded as a mere rule drawn from the combinations of protoxides with substances containing plural atoms of oxygen. (*Vide* First Principles, and Turner's Chemistry.) But this leaves out of view the numerous cases wherein a sesqui- or bin-oxide requires a proportionate number of atoms of acid; as for instance, sulphate of iron, in which, by the absorption of oxygen, two-thirds saturate the acid, and one-third precipitates: as well as other cases still more complex.

instead of 3; and boron 2·95 − 2 = ·95; which would lie between the numbers of these two eminent chemists; between the result of Berzelius's analysis of borate of ammonia, and Thomson's of borax; and between the analysis of the hydrate, in which they exactly accord, and that of Davy. All these experiments may be referred to (First Prin., article *Boron*).

(*e.*) Chlorine.—Although Berzelius's method of taking the undecompounded gases, hydrogen, azote, &c. in volumes, instead of equivalents, be somewhat incommodious, it was necessary, in giving his numbers, to conform to it; and the fractional expression is employed as readiest of comparison by neglecting the denominator.

It may be here observed, that direct experimental comparison of the atomic weights of chlorine and oxygen, by heating chlorate of potash very gradually in a platinum crucible over a spirit-lamp, before I recollected the experiments of Berzelius (Ann. Phil. xv. 91), gave me a number for chlorine rather less than his; although indirect trials, by double decomposition, gave a number nearly corresponding with that of Thomson.

(*f.*) Cobalt.—The number for this metal being calculated by Berzelius from experiments not his own, it seemed fair to incline the mean to Thomson's side.

(*g.*) Columbium.—This number being also altered, without stated reasons, in Berzelius's last table, and two-thirds of it, (indicated by his construction of the acid $\dot{C}b^2$) = 769·143, differing widely from Thomson's, even if doubled; these seemed no sufficient indications for altering the latter.

(*h.*) Fluoron (and Fluorine).—If the fluoric be an oxy-acid, its base must have a name; and the above is used as the readiest that occurs.

Berzelius found (Ann. Phil. xv. 280) that fluate of silver, "heated to redness, melted, and continued, as long as it was exposed to the fire, to give out fluoric acid and oxygen gases; whilst metallic silver was disengaged:" and he believed this was not owing to the presence of water. Even if it had been from that cause, it would be remarkable that so energetic a substance as the hypothetical fluorine should be so easily driven off; when iodine, under similar circumstances, bears a red heat. Fluoboric acid is explained on the fluorine hypothesis (First Prin. ii. 183), as "1 atom fluorine and 2 atoms boron;" although (at vol. i. 161) it is said "Davy's experiments on the composition of boracic acid; Berzelius's analysis of borate of ammonia, and mine of borax;—preclude the possibility of either more or less than an atom of boracic acid being united, in fluoboric acid, with an atom of fluoric acid."

There

There is an evident oversight here; and we can only reconcile fluoboric acid to the fluorine theory, by supposing it analogous to phosgene gas. If the oxygen theory be adopted, the base of fluoric acid will of course weigh two less than "fluorine."

(*i.*) Glucinum: subject to similar observations with aluminum (Note *a.*).

(*k.*) Mercury.—I have, on a former occasion*, offered some reasons for conceiving the black oxide and calomel a suboxide and subchloride of mercury; and consequently that the atom of metal is only half the weight assigned by Thomson. My reasons are shortly these: The red oxide is formed floating on the mercury, in calcination *per se*; the same oxide is produced by boiling mercury with sulphuric acid, in which process sulphurous acid gas is given off; and water decomposes the salts of this oxide, precipitating a portion of it, unless there be an atom of acid for every 12·5 parts of mercury. Solution of corrosive sublimate produces no effervescence with solution of carbonate of soda, though mixed boiling hot; nor, mixed in solution with oxalate of soda, evaporated to dryness, and subjected to distilled water, does it affect turmeric or litmus paper, unless the sublimate be in excess. And the most intimate combination of mercury with sulphur is cinnabar, composed of mercury 12·5, sulphur 2. 12·5 may not be the *precise* number, though I think nearer than that of Berzelius. 12·55 (Wollaston's number) is preferred.

(*l.*) Nickel.—See note on Cobalt.

(*m.*) Phosphorus.—The experiments of Dumas, corroborated by Buss (*Ann. Ch.* xli. 220), show that phosphuretted hydrogen contains 150 hydrogen in 100 cubic inches; and requires 200 oxygen to produce water and phosphoric acid.

150 of hydrogen consume 75 of oxygen; and the remaining 125, weighing 42·36 grs., require (from Berzelius's synthesis, phosphorus 100, oxygen 128; on a larger scale than Davy's) 33·1 of phosphorus; which must have been present in the gas. The condensation of the hydrogen, analogous to that in ammonia, would indicate the atomic proportion 3 : 1; and in that case the 125 oxygen (= to 250 hydrogen) will be 5 : 1. The atomic weight of phosphorus will then turn out

From the hydrogen	150 cub. in. of hydrogen weigh	3·177 grs.
	The combined phosphorus was found to be....................	33·1
	And $\frac{3{\cdot}177}{3}$: 33·1 : : 0·125 :	3·9 atom of phos.
From the oxygen	125 cub. in. of oxygen weigh	42.36 grs.
	The phosphorus................	33·1
	And $\frac{42{\cdot}36}{5}$: 30·1 : : 1	3·9 atom of phos.

* See Phil. Mag. and Annals, N.S. vol. vi. p. 167.—Edit.

Then

Then 3·9 phos. + 5· oxyg. will give 8·9 for the atom of phosphoric acid. But this quantity saturates two atoms of base, and therefore probably contains two atoms of phosphorus, according to Berzelius's formula ($\dot{\ddot{P}}h^2$). Thomson makes 9 phosphoric acid saturate 2 atoms of base; Berzelius 892·31. His number for phosphorus 196·155, being very nearly half of that found above, and the mean of the three (calculating Thomson's by the same formula) is adopted, neglecting the low decimals.

(*n.*) Rhodium, &c.—Berzelius has given in the 40th volume of the *Ann. Ch.* some elaborate *Recherches sur les Métaux qui accompagnent le Platine*, in which their atomic weights were ascertained by a very unexceptionable process,—reduction of the triple chloride, placed in a glass tube, over a spirit-lamp, by a current of hydrogen gas. The loss gives the chlorine; and the alkaline chloride being washed away, the metal is weighed. Soda-chloride of rhodium was used; and potash-chloride of the other four. His numbers will hardly require correction for the very different ones of Thomson, obtained by less satisfactory operations; but being calculated from chlorine, estimated at 442·65, they give (when adjusted to 4·46, the mean weight of that substance,) the numbers in the third column.

(*o.*) Silicon.—Berzelius's number is dependent on his Canon, before quoted; and the deductions of Thomson (First Prin., article *Silicon*) are so satisfactory, and his number is so convenient in application, as to claim preference over any result of hypothetical considerations.

(*p.*) Tungsten.—The difference between these numbers is so great, that one of them must in all probability involve some unobserved cause of error; but the results of the experiments (First Prin. ii. 62; and Ann. Phil. iii. 245) are so nearly equal, that it is not easy to discover with whom it lies. It seems better to go between the two, than to run the hazard of taking the wrong; and the mean is adopted from expedience merely.

(*q.*) Yttrium and Zirconium.—Yttrium is subject, in a less degree, to similar observations with tungsten; and zirconium also, with this difference, that the difficulty of choice consists in the experiments of Thomson being indirect, and those of Berzelius not given.

(*r.*) Zinc.—The difference here also is great; but Thomson's experiments are so comprehensive, and appear to have been so attentively conducted, that, notwithstanding the exceptions taken to them, his number seems entitled to more confidence than that of Berzelius, drawn only from the composition of the

oxide

oxide (Ann. Phil. iii. 358): and the mean taken is inclined conformably.

The insertion of any experiments of my own has been avoided, as needlessly swelling this paper, which proceeds on other grounds, and is already too long. For the same reason, the corresponding tables of oxides and acids are withheld; but if you find this deserving of publication, they can be forwarded at a future time. I am yours, &c.

Plymouth, April 10, 1830. JOHN PRIDEAUX.

XXVI. *Reply to the Statement respecting the Discovery of* Cygnus Bewickii, *published in the Phil. Mag. and Annals for August. By* W. YARRELL, *Esq. F.L.S.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

ALTHOUGH anonymous accusations can have little influence with your readers, and might therefore safely be treated with the neglect they merit; I yet, in self-defence, request insertion in your Magazine, of the following brief observations, in answer to a letter, addressed to you by a member of the Natural History Society of Newcastle-upon-Tyne, which appeared in your 44th Number, published at the commencement of the present month.

It is first necessary that I should justify the opinion I gave by letter, that the distinctions described as existing in the parts of two swans might be occasioned by "age, sex, or accidental circumstances." For this purpose I shall quote, *verbatim*, the description itself, as sent me in March 1829, at which time the opinion was given.

"A young man, of the name of ———, who has given considerable attention to the ornithological part of Natural History, has the breast-bone and trachea of the common wild swan and of the new one; and our joint observations are as follows:

"The new species has eight ribs, and the old one only seven; the two specimens of the breast-bone are the same length, ten inches from the extremity of the merry-thought (where it joins to the elbow of the neck-bone) to the other end of the breast-bone; but whereas in the common wild swan the bend of the trachea takes place seven inches from the extremity of the merry-thought; in the new one it is only four inches and a half. In the common wild swan the wind-pipe on entering the breast-bone is nearly round; in the new kind, so much flattened as to equal half an inch by a quarter. In the common

mon wild swan the junction of the wind-pipe with the bronchial pipes takes place just underneath the bend of the merrythought; but in the new one, exactly under the hind bend of the wind-pipe; and finally, in the new kind the bronchial tubes are much shorter. I am afraid I have only given a very bungling description, but I hope, such as it is, it may be acceptable. I quite regret I cannot draw sufficiently well to send you a sketch of the two."

Such is the description; and having stated it, I leave it to the opinion of any person conversant with the subject, whether I should have been warranted in deciding that the differences were such as would constitute a new species. How few are there even now, with the figures of both species before them, who would be able to recognise either from such a description! And was I not justified in supposing there might have been "error of observation"? To say nothing of the confusion which renders much of this description unintelligible, it is quite clear from what we now know, that in more than one part of it, for 'old species' we must read 'new species', and *vice versâ*.

The new light stated by your anonymous correspondent to have afterwards dawned upon me, he ought also in justice to have added, was in the month of November 1829, and was the legitimate consequence of the acquisition of new materials. And surely I cannot with fairness be accused of any endeavour to depreciate the merits of Mr. Wingate, whom I have never spoken to or even seen, when in December last I offered to him, thus wholly unknown to me except by name, the materials I possessed, to complete his paper; nor that I exhibited any intention of keeping him out of sight, when, after he had declined publishing, I included in my statement a notice of his discovery of the same bird, with a date ten months antecedent to my own paper.

I have reason to believe, from several circumstances, that there was a scarcity of materials at Newcastle, and that they were insufficient for framing a description of this bird, as regarded the internal construction.

At the end of December I received a letter from Mr. Selby, requesting to be allowed to make use of some observations in a letter of mine on the subject of this bird, with any further remarks, and the use of any drawings I possessed relating to this species, which, to use that gentleman's own words, he writes, "you so appropriately suggest should be named after our celebrated countryman the late Mr. Bewick." The materials offered Mr. Wingate I transferred to Mr. Selby, with all the additions, which, during the interval, had come to my knowledge,

knowledge, at the same time stating, that, as Mr. Wingate had declined publishing, and I had in the mean time obtained five additional examples of the new species, I had drawn up a paper from my own materials, which I had sent in to the Linnæan Society, with all the drawings I possessed on the subject. Mr. Selby's paper, I have no doubt, will be worthy his high and deserved reputation; and the more information we obtain of so interesting an addition to our native Fauna, the better.

I regret sincerely the necessity of referring to private correspondence; but the contents of letters have been quoted against me, and self-justification must be my apology.

I am, Gentlemen, yours, &c.

Ryder Street, St. James's, Aug. 10, 1830. Wm. Yarrell.

XXVII. *An Attempt to explain theoretically the different Refrangibility of the Rays of Light, according to the Hypothesis of Undulations.* By the Rev. J. Challis, *Fellow of Trinity College Cambridge, and of the Cam. Phil. Soc.**

THE object of this communication is to follow up an idea advanced by Dr. Young, to explain theoretically the different refrangibility of the rays of light. (Lectures on Natural Philosophy, vol. ii. p. 623.) His notions on this subject have not met with the attention they deserve, probably because they are vague, and are not supported by mathematical calculation. There is much plausibility in his leading idea, viz. that the velocity of propagation of the æthereal undulations which traverse any medium, is modified by the vibrations of the material atoms of the medium, and differently according to the different frequency of the undulations: but the precise manner of the modification is probably not such as he supposes, and is open to further consideration.

For the object proposed, it will be necessary to attend to the manner in which a series of undulations is reflected when they encounter an obstacle. Suppose the fluid to be such that the pressure is proportional to the density, $p = a^2(1 + s)$; and the portion of it we consider, to be included in a slender cylindrical tube. Let two series of undulations, equal in every respect, and generated under circumstances exactly alike, be propagated in opposite directions along the tube. By the principle of the coexistence of small vibrations, these undulations will be propagated in their respective directions with-

* Communicated by the Author.

out affecting each other. Hence there will be one point at least, at which the velocities arising from the two propagations will be equal and opposite, and consequently the resulting velocity be nothing. At this point suppose an indefinitely thin rigid partition to be placed transverse to the axis of the tube, so as to divide the fluid into two separate columns. By this supposition the state of the motion will in no respect be altered. But the column on one side of the partition cannot affect that on the other; therefore if the one be removed, the motion of the other will remain unchanged. In this case the partition serves as an obstacle against which the undulations are reflected; and we are thus taught that the reflected waves are exactly like the incident, and are in fact a continuation of the incident, but diverted into an opposite direction of propagation. Let the law of the velocity v and condensation s of the incident undulations be given by the equations,

$$v = -as = -m \sin \frac{\pi}{\lambda}(x + at),$$

in which x is measured from the plane of reflection, in the direction of the propagation of the reflected waves, and t is dated from an instant at which condensation commences at this plane. By changing the sign of a, we obtain the equations,

$$v' = as' = -m \sin \frac{\pi}{\lambda}(x - at),$$

applicable to the reflected waves. At any point distant by x from the reflecting plane, the velocity at a time t will be $v + v' = -2m \sin \frac{\pi x}{\lambda} \cos \frac{\pi a t}{\lambda}$, and the condensation $s + s' = \frac{2m}{a} \cos \frac{\pi x}{\lambda} \sin \frac{\pi a t}{\lambda}$. Hence if $\sigma =$ the condensation at the origin of x, $a\sigma = 2m \sin \frac{\pi a t}{\lambda}$.

I will just remark, that the foregoing method of solving the problem of reflection may be readily extended to motion in space of three dimensions, by conceiving undulations to be propagated under circumstances exactly alike from two separate centres, and the fluid to be divided by an indefinitely thin rigid partition, bisecting the straight line joining the centres at right angles. The principle of the *division of fluids*, as it may be called, which is here made use of, is legitimately employed, because the possibility of such separation of the parts of fluids, without affecting the state of motion or rest, forms a characteristic property by which they are distinguished from solids, and might be made the foundation of the mathematical treatment of them; since the equality of pressure in all directions

tions from a given point, which is usually taken for the fundamental principle, is deducible from this.

Having thus found in what manner a series of undulations is affected when incident upon an immoveable partition, let us consider what will take place if the reflecting plane be susceptible of motion, in such a manner, however, that its velocity is always very small compared to the velocity of propagation. Suppose the plane to be acted upon by a force $n\,x$, varying as the distance x from the place of rest. Its velocity at the time t is $\frac{dx}{dt}$, and the condensation on the side opposite to that on which the waves are incident will be $\frac{dx}{a\,dt}$. Hence a portion equal to this of the condensation of the incident waves will not be reflected, but will be transmitted by the motion of the plane: and thus the excess of condensation on the reflecting side of the plane will be $\frac{2}{a}\left(m \sin\frac{\pi a t}{\lambda} - \frac{dx}{dt}\right)$. Consequently the plane is acted upon by an accelerative force

$$-n\,x + k\left(m \sin\frac{\pi a t}{\lambda} - \frac{dx}{dt}\right),$$

k being a constant depending on the quantity of matter moved.

Hence, $$\frac{d^2 x}{dt^2} + k\,\frac{dx}{dt} + n\,x - k\,m \sin\frac{\pi a t}{\lambda} = 0.$$

This equation integrated in the usual way, gives

$$x = e^{\frac{-kt}{2}}(c \cos h\,t + c' \sin h\,t) -$$

$$\frac{k\,m}{\left(\frac{\pi^2 a^2}{\lambda^2} - n\right)^2 + \frac{\pi^2 a^2 k^2}{\lambda^2}}\left(\left(\frac{\pi^2 a^2}{\lambda^2} - n\right) \sin\frac{\pi a t}{\lambda} + \frac{\pi a k}{\lambda} \cos\frac{\pi a t}{\lambda}\right)$$

The term involving $e^{\frac{-kt}{2}}$ will soon disappear on account of the diminution of this factor as the time increases, and the motion will then be given by the equations,

$$x = -\frac{k\,m}{\left(\frac{\pi^2 a^2}{\lambda^2} - n\right)^2 + \frac{\pi^2 a^2 k^2}{\lambda^2}} \times$$

$$\left\{\left(\frac{\pi^2 a^2}{\lambda^2} - n\right) \sin\frac{\pi a t}{\lambda} + \frac{\pi a k}{\lambda} \cos\frac{\pi a t}{\lambda}\right\}$$

$$\frac{dx}{dt} = -\frac{\pi a}{\lambda}\cdot\frac{k\,m}{\left(\frac{\pi^2 a^2}{\lambda^2} - n\right)^2 + \frac{\pi^2 a^2 k^2}{\lambda^2}} \times$$

$$\left\{\left(\frac{\pi^2 a^2}{\lambda^2} - n\right) \cos\frac{\pi a t}{\lambda} - \frac{\pi a k}{\lambda} \sin\frac{\pi a t}{\lambda}\right\}$$

Suppose

Suppose that $\dfrac{\dfrac{\pi^2 a^2}{\lambda^2} - n}{\dfrac{\pi a k}{\lambda}} = \cot \varphi$. It will be found that $\dfrac{dx}{dt}$

$= - m \sin \phi \cos \left(\dfrac{\pi a t}{\lambda} + \varphi\right)$. Hence if $\sigma =$ the quantity of condensation reflected,

$$a\,\sigma = m \sin \frac{\pi a t}{\lambda} + \frac{dx}{dt}$$

$$= m \cos \phi \sin \left(\frac{\pi a t}{\lambda} + \phi\right)$$

We thus learn that when the reflecting plane is moveable in a small degree, in the manner above supposed, the reflected waves will be similar to the incident, but the condensations of the former will have to the corresponding condensations of the latter, a ratio $\cos \varphi$, which depends on the breadth of the waves, but is independent of the degree of condensation.

To apply the preceding result to the action of the waves of the æther on the particles of a medium through which they are propagated, it will be necessary to make some hypotheses respecting the constitution of the medium, and the state of the æther in its interior. I suppose, after M. Poisson, that the medium consists of exceedingly minute but finite atoms; so minute that the space they occupy is very small compared to the intervening spaces free of atoms, and yet so near each other that an immense number are disposed along a linear space equal to the mean value of λ for luminous waves, which is a 50,000dth part of an inch. Also for the sake of precision of idea I conceive the medium to be homogeneous, so as to contain a given number of atoms all of the same size and mass in a given space, and the atoms to be spherical in shape. With respect to the state of the æther in mediums, Dr. Young says, "It is simplest to consider the æthereal medium which pervades any transparent substance, together with the material atoms of the substance, as constituting together a compound medium denser than the pure æther, but not more elastic." At the same time he admits that "the phænomenon of aberration is not easily reconcileable with the theory of undulations, if the æther be carried along before the medium it pervades, and partake materially of its motion." This phænomenon seems to leave us at no liberty to suppose that the density of the æther is at all different in the interior of mediums from what it is in free space. For admitting that a medium in motion does not carry any portion of æther along with it, it is difficult to conceive how the spaces into which it is successively transported should receive a sudden accession of æthereal matter, and that too without any sensible effect being

being produced. Whatever hypothesis be adopted respecting the density of the æther in the interior of mediums, it will be proper to inquire in what manner the motions of the æthereal particles are modified by the presence of the atoms of the mediums; and this inquiry is peculiarly necessary on the supposition that the density is the same in mediums as in free space (which is the hypothesis we have selected, and is the simplest that can be made); for on this supposition the diminution of the velocity of propagation in mediums must be *solely* owing to the obstacle which their numerous and closely arranged atoms oppose to the free motion of the æthereal particles. Wherever the continuity or homogeneity of the mediums is interrupted, *sensible* reflection will take place; but in the interior of a uniform medium the cause of retardation will act uniformly, and its *mean* effect, on the supposition of uniform propagation, will be, to make the condensation corresponding to a given velocity greater in a certain proportion than in free space, and to diminish the velocity of propagation in the same proportion. This may be inferred from an investigation relative to this subject, which I gave in the Philosophical Magazine and Annals of Philosophy for May 1830; in which it was also shown, by reasoning on the above hypothesis, and on the single assumption of the uniformity of propagation, that the ratio m of the velocity of propagation in free space to that in a medium, may be found from the equation

$$\frac{m^2-1}{m\rho} = \mathrm{H},$$

in which ρ is the density of the medium, and H a constant proportional to the mean retardation of a given number of its atoms supposed immoveable. Hence if the atoms be susceptible of motion, so that when drawn a little from their positions of rest they tend to return by forces varying as the distances from these positions (and various phænomena make it probable that this is actually the case), the quantity H, in so far as the retardation is proportional to the reflective power of the atoms, should be multiplied by $\cos\phi$. For, without stopping to inquire the manner of reflection from a single atom, we may presume that the quantity of reflection from a given number thickly disposed in a plane superficies, will have a given proportion to the reflection from a continuous superficies of equal magnitude, and susceptible of the same kind of motion as the atoms. Hence,

$$\frac{m^2-1}{m\rho} = \mathrm{H}\cos\phi$$

Now, $\cot\phi = \dfrac{\dfrac{\pi^2 a^2}{\lambda^2} - n}{\dfrac{\pi a k}{\lambda}}$, and as $\dfrac{\pi^2 a^2}{\lambda^2}$, which is a quantity immensely

immensely large, will in all probability be greater than n, ϕ will be greater as λ is greater. Therefore m will be less as λ is greater. Hence the rays for which λ is greatest, that is the least refrangible rays, will be propagated with the greatest velocity. Also the velocity of propagation is independent of the intensity of the rays. These results are conformable with experience.

As n is probably exceedingly small compared with $\frac{\pi^2 a^2}{\lambda^2}$, it is possible that λ may have a value much larger than it has for luminous waves, and cot ϕ not change sign in consequence, but become very small. Then cos ϕ would $= 0$, and $m = 1$ nearly. Thus there may be æthereal undulations much broader than those which cause light, that do not undergo sensible change in their velocity of propagation, as they enter mediums.

XXVIII. *On the Magnetic Polarity of two Rocks of Basalt near Nürburg in the Eifel, with some Observations on the extension of Basalt in that district; drawn up from the Observations of* Bergmeister Schulze *of Düren. By* Professor J. Nöggerath.*

SINCE the discovery, by Alexander Von Humboldt, of the magneto-polar property of a rock of serpentine on the *Haidberg* or *Heideberg*, near Celle, in the country of Baireuth, many other rocks have been found possessed of the same property, viz.: serpentine, and rocks of other kinds, such as hornblende-slate, porphyry, trachyte, basalts, &c.†

It seems, however, to be found only in mountains containing magnetic iron-stone, although the quantity of this admixture in itself does not limit the intensity of this property; as indeed it shows itself with different purely magnetic ironstones in the greatest variety of degrees of strength, and there are some of these which show no magneto-polar action.

Nor does there appear, from all the observations made on magneto-polar rocks and fragments, that there is any regula-

* From Schweigger's Journal.

† For the discovery of Humboldt and those connected with it, see in particular the *Intelligenz Blatt of Jenaer Algemeine Literaturzeitung*, 1796, No. 169, p. 1447; *ibid.* 1797, No. 38, p. 323; No. 68, p. 564; and No. 87, p. 722. *Neues Bergmann's Journal*, i. pp. 257 & 542. Gren's *Neues Journ. d. Chem. u. Phys.* iv. Heft i. p. 136. V. Moll's *Jahrbuch d. Berg u. Hutten Kunde*, iii. p. 301. V. Moll's *Neues Jahrbuch d. B. u. H.* ii. p. 403. Gilbert's *Annalen*, neue Folge xiv. Heft i. p. 89. Goldfusz and Bischof's *Phisik. statist. Beschreibung d. Fichtelgebirges*, i. p. 139. and *ibid.* altere Reihe xviii. p. 297.

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rity in the position of the axes either in one and the same mass of rock in general, or a fixed correspondence, in the position of these axes, with the direction of the strata of the rocks. Individual cases are indeed found in which a number of traversing magnetic axes lie parallel in a rock, and even instances in which their position is in constant correspondence with the direction of the strata of the hills in which they are found; but there are found in the same rock, besides those parallel axes, others of a deviating position, so much so as to intersect and cross each other, and to meet within the rock with poles of the same description*.

But very recently I received an official report of the magneto-polar property of two basaltic rocks in the Eifel, which may be considered as an extraordinary instance of variety of position of the axes contained in them. On my request to Bergmeister Schulze of Düren to communicate to me his observations on them, I received the following letter from him, which, besides containing the information I sought, gives some other interesting particulars of the district of the Eifel.

I consider these two basaltic rocks as peculiarly adapted for making strict investigations on the polarity of rocks, as they are comparatively small, and may be examined on every side, and the variations of the phænomena are placed close, and yet very conspicuously, together. I shall employ my first leisure for their examination. For the present, a young but distinguished natural philosopher has promised to devote a few days to this purpose during the Easter vacations. If the results turn out generally interesting, I shall publish them. In the mean time the following letter may suffice.

"Düren, 18th February, 1828.

"Since you, my respected friend, consider the discovery I made during my last summer's excursion in the Eifel of some rocks of basalt having magnetic power, sufficiently important to be placed before the public, I shall have the honour to give you a short account of it. I only regret that it cannot be a

* The greatest constancy in the position of magnetic axes in polarizing rocks has probably been observed in that of the above-named Haidberg. But it only requires a close study of the observations made by the last examiners of it, Professors Goldfusz and Bischof, to discover that there must be in this rock also, besides a great number of parallel axes, others of a deviating position; a fact which becomes the more apparent if we compare the description of the properties of the whole rock with the experiments made by Prof. Bischof with detached pieces of it. See *Beobachtungen über die magnetischen Eigenschaften einiger Gebirgsarten des Fichtelgebirges*, by Dr. G. Bischof, in this (Schweigger's) periodical, old series, vol. xviii. p. 297, &c.) Compare also in particular the account accompanying that Essay, and observe that page 324, line 17, the word *south-west* appears instead of *south-east*; evidently through an error of writing, or of the press.

scientific

scientific one, as the journey had no scientific object, except as to mining, and a general knowledge of the rocks forming the surface.

"Although an attempt may be made to form several of the groups of basalt, with which the Eifel is scattered over, into lines, there is none so long, and of which the range is so close, as that round the Nürburg, one league and a quarter south of Adenau. Its direction is meridional, with a slight inclination towards the west. It begins in the S. near Bertrich (not far from the Mosel), with the remarkable formation of basalt and scoria, runs by Walmeroth, Ulmen, Horperath, over the *high Kellberg*, between the villages of Kellberg and Ursfeld, by the Nürburg, the detached group near Adenau, with outliers as far as the Hochthurn and Hasenberg, between Kirchsahr and Altenahr. With the exception of the Hohen-Acht, two leagues S.E. of Adenau, this line includes the highest masses, exceeding all the summits of grauwacké-slate, on which they stand; such are the Hoh-Kellberg, the Nürburg, and the Hochthurn. These cones rise between 1900 and 2000 Prussian feet above the level of the Rhine. There is no other appearance of volcanic origin in the cones of this chain (except at Bertrich and the *Maars* *) than in the basalts of the *Westerwald*, Hesse, and Silesia; and their nature would have remained in doubt if the closely aggregated and undoubtedly volcanic groups, running in a continuous range from the volcano of Bertrich as far as Hillesheim, did not bear the clearest evidence of it. I will not allude here to the volcanic range accompanying the Rhine, as it would draw me too far from my object, which is to show that the line of elevated basaltic cones here described, seven German miles in length, in their position nearly parallel with the magnetic meridian, may bear a relation to the peculiar magnetic property of the rocks of several of the hills of this line. I stood on the Michelstein, a double-pointed hill of basalt; on the northern mountain range of the Eifel, the beautiful semi-circle of the high cones of Hochthurn, Hohen-Acht, Nürburg, Kellberg, and Ahremberg, lay before me. The Nürburg rose in apparently a spiral form, which deception is increased by the ruined tower standing on its summit. Three days after I ascended it, I went up from Adenau as far as the Wimbacher-Höhe, between the village of Wimbach and Nürburg, where a promi-

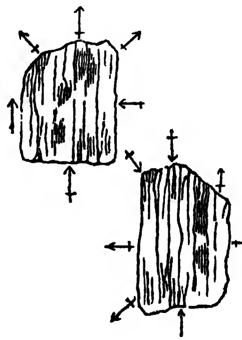
* The *Maars* are circular lakes among the *grauwacké-slate*, with steep banks. Without any visible supply, they overflow copiously through an aperture in their banks towards the neighbouring valley. Their highest edge is covered partly with a stratum of volcanic grit, mixed with scoria and pieces of slate, masses of augite and pieces of basalt, and partly with an alluvial deposit of trappean substances.

sing adit has been driven close. It consists of various fragments of rocks, and its course is parallel with the magnetic meridian, and consequently with the group of basalt of Nürburg. The mountain consists of grauwacké-slate, formed of beds of common grauwacké, slaty grauwacké, and clay-slate. By the trials since made, under the name of *Katharinengrube*, it is found to fall 70° S.; a dip which is as little constant as its direction. The great character of the strata is undulation, and the greater declivities run S. and S.E.

"The line on which the three highest cones, the Hohe-Acht, the Nürburg and the Hoch-Kellberg, are placed, is the south-eastern range of the Eifel, from the southern declivities of which the waters stream towards the Mosel and Rhine, but from the north-western declivity, towards the Ahr.

"It seems as if the basaltic cone of Nürburg had afforded some protection to the grauwacké-slate, as the latter gently rises even to the spot whence the former abruptly ascends. The mass of basalt is indeed small when we consider the view it affords from a distance; but on the other hand the highest cone is surrounded by several smaller ones, some of which, such as the Selberg, at its northern foot, near Guiddebach, equal it in bulk. This Selberg rises from the bottom of the valley without, its top reaching the elevation of the borders of the valley. Other cones of a gray colour lie on the southern declivity of the same valley to the N.E. of the Nürburg; towards the E., at a distance of about 150 fathoms, rises on the table-land of the mountain chain a low gentle acclivity, with low rocks, called the Stein-Ecke, and a quarter of a German mile, towards the S.W., rises a remarkable cone. From this cone several very small masses of basalt run in a line towards the Nürburg, as it were marking the throwings up of an adit. Except the Stein-Ecke, all these masses of basalt have nothing to distinguish them from others of a similar formation. Looking from the top of the Nürburg, I perceived at first something on the eastern flat hill resembling some unimportant ruins of a building. Instead of ruins, however, I found two insignificant rocks, about half a fathom distant from each other in their diagonals, about one fathom high, and holding each half a square fathom in their base. The south-eastern rock is one fathom long, half a fathom broad; the other to the N.W. a little shorter, but broader. Both rocks are in strata, or rather are separated into thick slabs, as is seen in flinty-slate, which slaty structure is parallel to the long sides. Their dip is twelve hours, and therefore parallel to the basaltic range on which they lie, as well as to that of the metallic ore mentioned above. The separating surfaces fall 73° E. This inclination does

does not seem to be confined to the rocks as far as they are exposed, but probably descends deeper into the mountains, as would seem from an uncovered part I noticed, lower down on the south side. I marked the direction of the slaty beds, and communicated them to the *Ober-einfahrer*, Mr. Becher of Commern, who accompanied me; on his doing the same, we discovered a difference. Our attention being thus excited, I went round both rocks, and holding the compass everywhere close to them, noted the directions of the needle, as represented in the annexed figure*. The variations of the needle were always sudden and violent, so that its vibrations were few, but rapid. The N.W. rock seems to contain a magnetic axis, but not the other; in the meridian it attracts the northern point of the needle, and on the cross line running through the centre, the southern point turns towards the rock.



"The colour of the mass is dark gray, in some parts iron black, on the cross fracture conchoidal, without betraying to the naked eye any foreign admixture, such as magnetic ironstone. It is probable that there are other points about the surrounding basalt hills where the needle would be disturbed; but want of time, and unfavourable weather, prevented my making further experiments.

"From the Nürburg I went to the village of Kellberg, and looked the following day for trachyte porphyry with crystals of albite, about the cone called Freienhäuschen. Here, between the hamlet of Köttelbach and the Hoch-Kellberg, are two cones close together, to the N. the Brink, and to the S. the Frauen, or Freienhäuschen. On the first the mass of porphyry is of two kinds; the one apparently prevailing is of a liver-colour with delicate admixed parts. This latter mass is very distinctly formed in strata, and attached to, or more probably inserted in the other, dipping twelve hours, and dipping pretty strongly to the E. Too much engaged in looking for pieces of porphyry with remarkable crystals of albite, I suffered the weak attractive power of the black trachyte to escape my notice, on the spot, not perceiving it till afterwards in some pieces I had taken with me. The strata in the Brink, where

* It need perhaps scarcely be noticed, that the arrows in the figure pointing towards the rock indicate a northern, and turned from the rock, a southern attraction, and that the needle lying parallel to the rock is to mark an indifferent position.—N.

they

they are perceptible as well as the hills themselves, are meridional; and a quarter of a mile more south, is the pond of Morbruch, a *maar* of some extent. SCHULZE."

Addition to the foregoing Article:—On the Magnetic Polarity of two Basaltic Rocks in the Lordship of Schröckenstein. By M. REUSS *of Berlin, Counsellor of Mines.**

To the very interesting discovery of magnetic polarity in two rocks of basalt near Nürburg in the Eifel, published by Mr. Schulze of Düren, in the *Jahrb. der Chem. u. Phis.* (see the preceding article in the Phil. Mag. and Annals) may be added another made by myself in 1827, near the high Wostrai, in the lordship of Schröckenstein, in the Mittelgebirg.

This high Wostrai forms in itself a small, rather insignificant cone, but which, owing to its high situation, overlooks the whole neighbourhood, and affords an excellent prospect of a great part of the Mittelgebirg on both sides of the Elbe. Its elevation above the river near Aussig may be about 1800 feet. This cone is covered with wood to its summit, and is precipitous on all sides; only the top shows any rock uncovered, and forms a narrow *coombe*, falling from south to north. In the steeper parts the basalt is separated into tabular masses; these tables, which are about six or eight inches thick, fall four or five hours under nearly 24° W. On the west, the Wostrai connects itself with the Skala, or rather forms the highest point of it, and runs out in the southern and eastern gentle and well cultivated declivity of the *Gemeindeberg;* on the east the foot of the cone falls in the equally well cultivated plain belonging to the village of Malschen.

The basalt is of a *dark grayish black*, very *fine-grained* in the fracture, and contains very numerous but very small crystals of pyroxene. There is no visible trace of magnetic ironstone, which is always distinguished by its metallic lustre. Its polarity is remarkable, being so great that the needle at the eastern foot of the basalt rock is moved 40°, and at the cone itself 90° W. At the western foot of the rock the contrary is the case; but this polarity is shown not only in the whole mass of the rock, but likewise in the larger detached pieces, and even in the smallest fragments, the north point of the needle being at one end distinctly attracted, and at the opposite end as distinctly repelled.

The same polarity I discovered on the *Breitenberg*, which rises to the south of the great Wostrai, about 10° to 15° higher, is entirely covered with high forest trees, and presents only on the

* From Schweigger's *Journal.*

the eastern declivity detached low masses of rock, the nearer circumstances of which, respecting their position, are not known. Higher masses of rock project at the north-western declivity. The basalt forming them is also of a dark grayish black, somewhat coarser in the grain, but contains instead of pyroxene a great quantity of olivine, dispersed in very small grains of olive-green and bright vine-yellow colours. It also disturbs the magnetic needle, but less than that of the high Wostrai; and its polarity only shows itself when the needle is brought very close to the rock, whilst at the former it is moved at a much greater distance.

XXIX. *On the Composition of Chloride of Barium.* By Dr. Edward Turner, *Professor of Chemistry in the University of London.**

In taking a review of the present state of chemistry;—of the numerous compounds that have been discovered within a very limited period, and of which many have as yet been but partially or imperfectly examined;—of the results, often discordant, which analysts have obtained;—and of the opposite theoretic views which are prevalent, it is difficult to avoid suspecting the propriety of opinions that have been thought to rest on the sure basis of correct observation, or doubting the accuracy of analyses conducted by chemists of the highest reputation. The æra of brilliant discovery in chemistry appears to have terminated for the present. The time is arrived for reviewing our stock of information, and submitting the principal facts and fundamental doctrines of the science to the severest scrutiny. The activity of chemists should now, I conceive, be especially employed, not so much in searching for new compounds or new elements, as in examining those already discovered; in ascertaining with the greatest possible care the exact ratio in which the elements of compounds are united; in correcting the erroneous statements to which inaccurate observation has given rise; and exposing the fallacy of opinions which partial experience or false facts have produced. Considerable as is the labour and difficulty of such researches, they will eventually prove of great importance to chemical science by supplying correct materials for reasoning; and will sometimes, even in the most familiar parts of analytical chemistry,

* From the Philosophical Transactions for 1829, part ii. Although some time has now elapsed since the publication of this paper, we still think it right to transfer it to our pages, on account of the connection of the subject with those of several communications inserted at various recent periods in the Phil. Mag. and Annals.—Edit.

mistry, lead to the detection of errors that had escaped notice, and which vitiate many analyses previously regarded without suspicion. An instance of this kind I shall have occasion to notice in the present communication.

The foregoing reflections have been more immediately elicited by circumstances connected with Dr. Thomson's "First Principles of Chemistry." The celebrated author of that work has attempted to ascertain the equivalents of all elementary substances; and as the result of his labours, has inferred the truth of an ingenious conjecture, suggested some years ago by Dr. Prout, that the weights of the atoms of bodies are simple multiples of the atomic weight of hydrogen. (Annals of Philosophy, vol. vi. p. 321.) This hypothesis is of so much importance if true, and may give rise to so much error if false, that its accuracy cannot too soon be put to the test of a minute experimental inquiry. The only chemists who to my knowledge have objected on experimental grounds to Dr. Thomson's support of this hypothesis, are Dr. Ure and Berzelius; but unfortunately both these gentlemen have written on the subject with such acrimony, and assumed a tone so unusual in scientific controversy, as in a great degree to have destroyed that confidence which their well-founded reputation for sagacity and skill would otherwise inspire. The uncertainty in which this question is still involved, has induced me to investigate it; and the essay which the Royal Society do me the honour to hear this evening may be viewed as the commencement of a series of essays designed for the elucidation of the same subject. As I shall have occasion on individual points to differ repeatedly from Dr. Thomson, I embrace this opportunity to declare, that in considering his statements with the freedom required for eliciting truth, I bear towards him no other personal feelings than those of kindness for civility received at his hands, and of respect for a man who has devoted his life zealously and successfully to the promotion of science.

The object of the present essay is to determine the composition of chloride of barium. The frequent employment of this compound in chemical experiments renders an exact knowledge of its constitution peculiarly important; and it has been used so extensively by Dr. Thomson as a medium of analysis, that an examination of it will afford an excellent criterion of the accuracy of his researches. Dr. Thomson has employed chloride of barium in ascertaining the equivalent of sulphuric acid, and of not less than thirteen metals and their protoxides; so that if his examination of this substance is inexact, the error will probably affect a large portion of his treatise. Dr. Thomson has been led by his observations to adopt 36 as the equivalent

valent of chlorine, 70 as that of barium, and 78 as that of baryta. The equivalent of chloride of barium is therefore 106; and on mixing this quantity of the chloride with 88 parts of sulphate of potash, each being previously dissolved in separate portions of distilled water, he finds that the clear liquid left after the insoluble sulphate of baryta has completely subsided, is not rendered turbid either by muriate of baryta or sulphate of soda. It is hence inferred, that by double decomposition the whole of the baryta has united with all the sulphuric acid, and that all the potash and muriatic acid are contained in solution in the form of muriate of potash. The resulting sulphate of baryta, after being collected and heated to redness, weighed exactly 118 parts; while the muriate of potash, when collected and duly heated, yielded 76 parts of chloride of potassium. It follows from this experiment that 40 is the equivalent of sulphuric acid, and 48 of potash; and on mixing with one equivalent of chloride of barium such a quantity of any soluble sulphate as should produce a similar interchange of elements, the constitution of that salt would be exactly determined.

This leading experiment, from which Dr. Thomson deduces the composition of chloride of barium as well as the atomic weight of baryta, is maintained by Berzelius to be inexact. He prepared chloride of barium and sulphate of potash with the greatest possible care; and on mixing them in the proportion mentioned by Dr. Thomson, he found that a considerable quantity of the former, about 2·25 per cent. of the amount employed, remained free in the residual liquid. (*Lehrbuch der Chemie*, vol. iii. p. 106.) In an answer to this objection, published in the Philosophical Magazine and Annals of Philosophy for last March, Dr. Thomson has maintained the accuracy of his original experiment, stating that it had recently been repeated by six of his practical pupils, and in no case did the residual liquid contain a trace either of sulphuric acid or baryta. I regret that my observations have forced me to a conclusion precisely opposite. I have made the experiment in question repeatedly, with the greatest care, and with perfectly pure materials, and in every instance the result coincided with that obtained by Berzelius. The sulphate of potash which I used was prepared by repeated crystallization from the crystals of that salt as sold by the druggists, and was so pure that I could not detect in it a trace of foreign matter. The chloride of barium was formed by the action of pure muriatic acid on native carbonate of baryta. The resulting solution was rendered alkaline with pure baryta, in order to precipitate any oxide of iron or manganese which might be present; and the crystals subsequently obtained by evaporation were reduced to powder,

powder, boiled in successive portions of alcohol, and fused. The fused chloride was redissolved in distilled water, and again obtained in crystals. This salt dissolved without residue or turbidity in water, and the solution was not affected by pure ammonia; it was not discoloured by sulphuretted hydrogen, hydrosulphuret of ammonia, or chloride of lime; when precipitated by an excess of sulphate of potash, the soluble parts were not rendered turbid by an alkaline carbonate or oxalate of potash; and when thrown down by pure sulphuric acid, evaporated, and ignited, the dry mass did not yield a trace of any soluble sulphate to water. Both compounds were heated to redness before being employed; and the chloride of barium, which if perfectly anhydrous, attracts moisture freely from the atmosphere, was always placed while hot in a weighed bottle secured by a tight cork, and its weight ascertained when cold. This precaution is not necessary with sulphate of potash.

I have thought it right to enter into these details, not only that chemists may judge of the accuracy of my experiments by the care with which they were conducted, but because the error committed by Dr. Thomson appears referable to the neglect of some of these precautions. This opinion seems the more probable, since Dr. Thomson is uncertain whether in his original experiments he did not employ the muriate of baryta of commerce, and if so he doubtless must have operated with an impure substance. But independently of any inaccuracy arising from this source, I shall now endeavour to prove that his method involves an error which precludes an exact result even with the purest materials. When solutions of muriate of baryta and sulphate of potash are mixed together, a small portion of the latter invariably escapes decomposition, and falls tenaciously adhering to the sulphate of baryta. I was led to this fact by observing, that when a known quantity of chloride of barium is precipitated by sulphate of potash, the resulting sulphate of baryta always weighed more than when the precipitation was made with pure sulphuric acid. The appearance of the salts after exposure to a red heat, was likewise different; the impure sulphate being harder, more brittle, and less opaque than the pure sulphate. The former reduced to powder and boiled with water, yielded a solution which precipitated barytic salts freely, and afforded certain evidence of the presence of potash with muriate of platinum.

The presence of sulphate of potash was at first naturally ascribed to imperfect edulcoration; but as it was still found, even after the precipitate had been washed with unusual care, I was led to examine the subject minutely. A solution of sulphate of potash was mixed with a large excess of muriate of

of baryta; the insoluble sulphate was edulcorated until the washings ceased to contain a trace of baryta, and was then collected on a filter, and ignited. On boiling it in powder with water, sulphate of potash was dissolved. The experiment was varied by mixing the solutions at a boiling temperature, and continuing the ebullition for some minutes; but the result was the same as before. On edulcorating the precipitate with boiling water, sulphate of potash begins to make its appearance in the washings as soon as the excess of muriate of baryta has been removed; but neither by this means, nor by boiling the recent precipitate for hours in successive portions of distilled water, have I succeeded in removing all the sulphate of potash. The adhesion of this salt ensues even in a dilute solution; and it is not prevented by the presence of other salts, such as nitre, and nitrate or muriate of ammonia, nor by free muriatic acid. The quantity of adhering sulphate of potash is variable, depending apparently as well on the relative quantity of the two salts, and the strength of the solution, as on the manner and extent of edulcoration. I have known it to increase the weight of the sulphate of baryta by one per cent.

The foregoing observations, unless I am much deceived, will fully justify the statement, that Dr. Thomson's method of analysing chloride of barium is radically defective. For if chloride of barium and sulphate of potash be mixed in the proportion to make a perfect interchange, some of the former will remain in the liquid, proportional to the quantity of the latter which escapes decomposition; whereas the absence both of sulphuric acid and baryta from the liquid can only occur, when the quantity of chloride of barium is insufficient for effecting complete double decomposition with the sulphate of potash. So that when the proportions appear to be right, they are certainly wrong; and they may be right, when they appear to be wrong. It is obvious, too, that Dr. Thomson's analysis of sulphate of potash by means of chloride of barium, is not more satisfactory than his analysis of chloride of barium by sulphate of potash. The equivalent of potash, deduced from that analysis, cannot be relied on; and his proof of 40 being the exact equivalent of sulphuric acid is also liable to objection. But the error upon which Dr. Thomson has so unhappily fallen, has been also committed by other chemists. Every analysis of sulphate of potash, or of salts containing this alkali and sulphuric acid, must be regarded with suspicion. Thus the analysis of common alum by Dr. Thomson and Berzelius can scarcely be quite exact; and the analysis of potash-minerals, in which baryta has been separated by sulphuric acid, may also be suspected of slight inaccuracy.

The

The process by which I have endeavoured to analyse chloride of barium consists of two parts. In the first, a given quantity of the chloride was dissolved in water, and the baryta thrown down as sulphate by sulphuric acid. In the second, a similar solution was precipitated by nitrate of silver, and the chlorine inferred from the quantity of fused hornsilver which was produced. The quantity of chloride of barium employed in each experiment varied from 30 to 40 or 45 grains. The sulphuric acid had of course been purified by distillation, and left no residue when evaporated on platinum.

The process by sulphuric acid was varied: one while the solution and precipitate were evaporated to dryness in a platinum capsule; and at another, the insoluble sulphate was collected on a double filter. Both methods were frequently repeated, and the sulphate of baryta was always dried by exposure to a red heat. The quantity of sulphate of baryta obtained by the first method from 100 parts of the chloride ranged from 112·17 to 112·2, being more frequently the latter than the former; and 112·19 may be adopted as a mean of the most successful experiments. The quantity obtained by filtration fell rather short of this, varying in the best experiments from 112·08 to 112·12. The difference is referable to a trace of sulphate of baryta being retained by the acid solution, in which it may really be detected by evaporation. The first series of experiments may therefore be considered the more accurate, and it may be inferred that 100 parts of pure chloride of barium are capable of yielding 112·19 parts of sulphate of baryta. This result agrees very closely with that stated by Berzelius in the last edition of his System of Chemistry, who in one experiment got 112·17, and in another 112·18, of sulphate from 100 parts of chloride of barium. According to Dr. Thomson, 100 parts of the chloride yield only 111·32 parts of sulphate of baryta. It is proper to state, in reference to the foregoing experiments, that traces of chloride of barium are apt to adhere to the sulphate of baryta; but this source of error is easily avoided by decanting the supernatant fluid after subsidence, and stirring the precipitate with hot water acidulated with sulphuric acid.

In order to determine the chlorine of chloride of barium by means of silver, it was desirable to ascertain the composition of hornsilver. For this purpose some fine silver containing only traces of gold and copper was dissolved in nitric acid, precipitated by sea-salt, digested in dilute nitro-muriatic acid, and washed. The dry chloride was then reduced by means of carbonate of potash in the usual manner, and after throwing a few fragments of nitre upon the fused metal, it was granulated and then boiled repeatedly in distilled water.

In the silver thus prepared I could not detect potash, gold, copper, or any other impurity; whereas it is difficult, in employing common silver, to purify it completely by one operation.

1. Of this silver 28·407 grains were dissolved in pure nitric, and precipitated by pure muriatic acid, both of which had been prepared with the greatest care. The whole mass was evaporated to dryness, and yielded 37·737 grains of fused chloride of silver.

2. In a second similar experiment 41·917 grains of silver yielded 55·678 grains of hornsilver.

3. In a third, 40·006 grains of silver yielded 53·143 of hornsilver.

According to the first and third experiments 100 parts of silver correspond to 132·84, and according to the second to 132·83 parts of hornsilver.

4. In a fourth experiment, 30·922 grains of silver were dissolved in nitric acid, and precipitated by muriate of baryta in excess. The precipitate after being carefully washed and collected on a double filter, yielded 41·07 grains of fused chloride; and hence the silver and chloride are in the ratio of 100 to 132·82.

5. In a fifth experiment, 42·255 grains of silver were dissolved as usual, precipitated by an excess of muriatic acid, and collected on a double filter. The fused chloride amounted to 56·09 grains, giving the proportion of 100 to 132·74. When the silver is thus precipitated by free muriatic acid, and the chloride collected on a filter, the result is constantly below that obtained by the other methods, owing to a trace of the chloride being dissolved by the strong acid solution.

It may be inferred, as a mean of the four first experiments, that 100 parts of silver correspond to 132·83 parts of chloride of silver. The proportion stated by Berzelius is 100 to 132·75; and it is estimated at 100 to 132·72 by Dr. Thomson. All these results, therefore, are closely correspondent.

From one of the experiments (No. 4.) just mentioned, it is manifest that the precipitation of chloride of barium by nitrate of silver does not involve any appreciable source of error. To be quite certain, however, as to this fact, chloride of barium was mixed with nitrate of silver in excess, and the precipitate carefully washed. It was then boiled in distilled water, and the fluid examined for silver and baryta; but not a trace of either could be detected. It dissolved completely in ammonia, and the addition of sulphuric acid did not cause the slightest turbidity.

In five analyses made by precipitating chloride of barium by an excess of nitrate of silver, I obtained the following proportions.

Chlorite

	Chloride of Barium.	Chloride of Silver.
Exp. 1.	100 yielded	137·45
2.	100	137·54
3.	100	137·70
4.	100	137·62
5.	100	137·64

Though all these analyses were made with great care, the last two were the most successful, as being less influenced by errors of manipulation than the others. Instead, therefore, of taking the mean of the five, which is 100 to 137·61, I adopt the mean of the two last experiments, which is 100 to 137·63. In one of these the precipitate was washed with distilled water only, and in the other with water acidulated with nitric acid. Berzelius in his experiments on this subject found that 100 parts of chloride of barium corresponded to 138·06 in one experiment, and 138·08 in another. This is the only material difference between us which I have yet had occasion to notice. It induced me to reconsider every part of my experiments; but as I am unable to detect the slightest inaccuracy in the two analyses from which my result was derived, I cannot hesitate to adopt it. I conclude, accordingly, that 100 parts of chloride of barium correspond to 137·63 parts of chloride of silver; and as, consistently with the preceding researches, this quantity of hornsilver contains 34·016 parts of chlorine, it follows that chloride of barium consists of

Barium	65·984
Chlorine	34·016
	100·000

Its constitution according to Dr. Thomson and Berzelius is shown by the following numbers:

	Thomson.	Berzelius.
Barium	66·037	65·926
Chlorine	33·963	34·074
	100·000	100·000

It is impracticable, from the composition of chloride of barium as above stated, to make any satisfactory inference relative to the real equivalent of barium, because the real equivalent of chlorine is not yet clearly ascertained. By Dr. Thomson it is estimated at 36, and by Berzelius at 35·43; and on calculating the equivalent of barium according to both estimates, the following result will be obtained.

Barium	69·832	68·726
Chlorine	36·000	35·430
	105·832	104·156*

* Another recent analysis of chloride of barium will be found in our last volume, p. 277.—Edit.

Hence

Hence if 36 is the equivalent of chlorine, that of barium will be 69·832, or very near 70 as stated by Dr. Thomson; but if the calculation be continued, still taking the results of my experiments as its basis, the equivalent of sulphuric acid will turn out to be 40·901 instead of 40. From these considerations it appears evident that at least one of the equivalent numbers concerned in the calculation must be incorrect. I abstain, however, from offering any further opinion on this point at present, as it will form the subject of another communication.

XXX. *On the Measurement (by Trigonometry) of the Heights of the principal Hills of Swaledale, Yorkshire.* By John Nixon, *Esq.*

[Continued from page 128.]

At Bakestone Edge.

June 15th, 1829. Height of Eye 3·5 feet.

AN alternately bright and cloudy afternoon, attended with gusts of wind from the south-west. The observations were discontinued, in consequence of the occurrence of a storm of heavy rain and mist.

		°	′	″		Feet.		″
	Satron Hangers ...	0	47	16	dep.	145·6	lower.	1·5
	The Tail Brigg ...	0	12	30	...	127·6	...	2·5
	E. Stonesdale Moor	0	9	11	...	56·6	...	3·5
	Keasdon............	0	54	8	...	284·0	...	4·5
(1828)	Pickington Ridge...	0	12	20	...	66·9	...	9
	Shunnor............	0	53	14	elev.	...	...	1·5
(1828)	———............	0	53	9	...	428·3	higher.	5
	Water Crag.........	0	25	28	...	...	...	2·5
(1828)	———.........	0	25	10	...	268·6	...	4
	Nine Standards Hill	0	13	22	...	250·6	...	1·5
	Hugh Seat	0	26	56	...	407·1	...	0·5

*** The height of the eye in the observations of 1828 was 4·5 feet.

At Shunnor Fell.

June 17th, 1829. Height of Eye 4·5 feet.

A beautifully clear afternoon. Light clouds, alternately from the north-west and south-west, passed over, almost without rain, but the horizon eastward had generally the appearance of heavy showers.

Hugh

		°	′	″		Feet.		″
	Hugh Seat..........	0	5	44	depr.	...	...	4
(1828)	——............	0	5	44	...	18·7	lower.	6
	The Tall Brigg ...	1	5	13	...	551·4	...	0·5
	Nine Standards Hill	0	23	5	...	175·0	...	0
	Rogan's Seat.......	0	19	26	...	143·6	...	0
	Water Crag.........	0	18	31	...	...	...	0
(1828)	——.........	0	18	19	...	157·5	...	7
	Great Pinch Yate ..	0	37	56	...	434·0	...	7
	Satron Hangers...	0	59	20	...	573·0	...	1·5
	Pickington Ridge...	0	38	52	...	...	...	5·5
(1828)	——...	0	39	12	...	495·9	...	9
	Harker..............	0	54	59	...	820·4	...	1
	Keasdon	2	14	12	...	712·1	...	1
	The Calf............	0	12	14	...	130·4	...	2
	Wildboar Fell......	0	5	42	...	...	...	1·5
(1828)	——......	0	5	34	...	23·4	...	5
	Bakestone Edge ...	0	58	28	...	...	...	3·5
(1828)	——...	0	58	53	...	...	...	7
(1828)	——...	0	58	43	...	427·6	...	12
	Pen-y-gent, *Wall top*	0	8	56	...	68·9	...	1
	Ingleborough ,.......	0	5	14	...	22·4	higher.	6
	Whernside..........	0	1	29	...	66·7	...	4·5

⁎ The height of the eye in the observations of 1828 was 4 feet.

To account for the great difference in the two observed depressions of Pickington Ridge, it is to be remarked that in 1828 the excessive haziness, and again, in the following year, the light mists skirting the dense rain in that direction, rendered the observations very uncertain.

The signal on Bakestone Edge is a grit tower placed close to the east side of, and not much above the level of the coal road formed of the same materials. Viewed from the greatly superior height of Shunnor Fell, the western side of the road, in favourable circumstances of light and shade excepted, would certainly be mistaken for the base of the signal. This source of uncertainty, added to those just mentioned, may possibly have led to the important discrepancies which the observations present.

In both cases it is singular that the mean differences of level come out within a foot of the truth.

At Rumbles Moor.

Height of Eye 4 feet.

		°	′	″	
(1821)	Great Whernside...............	0	26	18	elev.
(1822)	——..............	0	26	22	...
(1823)	——...............	0	26	15	...

At

At Great Whernside.

May 16th, 1829. Height of Eye 4 feet.
A calm, clear afternoon, succeeded by frost.

		°	′	″		Feet.		″
	Rumbles Moor	0	41	2	depr.	...	...	52
(1821)	————	0	40	58	...	...	...	...
(1822)	————	0	40	59	...	...	...	...
	Cam Fell	0	24	49	...	383·2	lower.	2
	YockenthwaiteMoor	0	21	10	...	199·4	...	3·5
	Dod Fell	0	11	28	...	118·5	...	5
	Settronside, *Wall top*	0	3	20	...	5·0	...	2·5
	Bakestone Edge.....	0	23	48	...	387·4	...	1
	Whernside	0	2	34	...	106·4	higher.	0·5
	Shunnor Fell	0	5	24	...	...	...	2
	————	0	5	10	...	...	...	4
	————	0	5	4	...	40·9	...	2·5
Eye 1 ft.	Ingleborough	0	3	53	...	...	...	5
	————	0	3	41	...	...	...	4
	————	0	3	38	...	62·2	...	4·5

At Ingleborough.

September 2d, 1829. Height of Eye 4 feet.
A cold and partially cloudy day. Objects remarkably distinct.

		°	′	″		Feet.		″
	GreatWhernside	0	8	56	depr.	...	...	1
	————	0	8	57	...	...	...	0
(1822)	————	0	8	59	...	65·2	lower.	...
	Shunnor Fell	0	7	6	...	...	...	1
(1822)	————	0	7	9	...	23·6	...	...
	The Calf	0	12	34	...	151·6	...	1

At Knoutberry Hill.

July 5th, 1827. Height of Eye 4 feet.

	°	′	″		Feet.	
Wildboar Fell.................	0	7	1	elev.	120·6	higher.
Hugh Seat.....................	0	5	15	...	123·3	...
The Calf........................	0	3	12	depr.	14·5	...

At Whernside.

July 4th, 1827. Height of Eye 4 feet.

	°	′	″		Feet.	
The Calf........................	0	16	29	depr.	196·9	lower.
Wildboar Fell................	0	10	4	...	92·2	...

At

At Pen-y-gent.

July 11th, 1827. Height of Eye above the *Wall top* 9 inches.

	°	′	″		Feet.	
The Calf....................	0	9	4	depr.	60·0	lower.
Hugh Seat....................	0	5	8	...	41·2	higher.
Wildboar Fell................	0	4	42	...	42·9	...

At Dod Fell.

August 25th, 1827. Height of Eye 4 feet.

	°	′	″		Feet.	
Hugh Seat....................	0	4	9	elev.	142·2	higher.
Wildboar Fell................	0	4	24	...	139·0	...

Calculation of the Mean *differences of Level and Heights of the Stations.*

Standard Heights	Ingleborough.....	2374·6 feet.
	Shunnor Fell.....	2351·3
	Great Whernside	2310·0

Bakestone Edge below *Ingleborough.*

	Feet.
By obs. at Settronside	447·6
——— Great Whernside	449·6
——— Shunnor Fell	450·0
——— Bear's Head	450·9
Mean	449·5
Height of Ingleborough	2374·6
——— *Bakestone Edge*	1925·1

Bakestone Edge below *Great Whernside.*

By obs. at Settronside	383·4
——— Shunnor and Great Whernside	386·7
——— Bakestone Edge & Do.	387·1
——— Penhill	389·2
Mean	386·8
Height of Great Whernside	2310·0
——— *Bakestone Edge*	1923·2

Bakestone Edge below *Shunnor Fell.*

By obs. at Settronside	425·4
——— Water Crag	427·7
——— reciprocal	428·0
——— at Great Whernside	428·3
——— Penhill	429·3
——— Pickington Ridge	429·8
——— Keasdon	429·9
——— Bear's Head	430·2
——— Great Pinch Yate	432·7
Mean	429·0

	Feet.
Height of Shunnor Fell	2351·3
——— *Bakestone Edge*	1922·3
Do. compared with Gt. Whernside	1923·2
——— Ingleborough	1925·1
Mean	1923·2

Pickington Ridge below *Ingleborough.*

By obs. at Shunnor Fell	518·3
——— Bear's Head	519·2
Mean	518·7
Height of Ingleborough	2374·6
——— *Pickington Ridge*	1855·9

Pickington Ridge below *Great Whernside.*

By obs. at Bakestone Edge	452·3
——— Penhill	453·0
——— Shunnor and Great Whernside	455·0
Mean	453·4
Height of Great Whernside	2310·0
——— *Pickington Ridge*	1856·6

Pickington Ridge below *Shunnor Fell.*

By obs. at Penhill	493·1
——— reciprocal	495·0
——— at Bakestone Edge	495·2
——— Water Crag	495·5
——— Great Pinch Yate	498·2
——— Bear's Head	498·5
Mean	495·9

Height

	Feet.
Height of Shunnor Fell	2351·3
——— *Pickington Ridge*	1855·4

Pickington Ridge below *Bakestone Edge.*

By obs. at Penhill	63·8
——— Great Pinch Yate	65·5
——— reciprocal	65·6
——— at Water Crag	67·8
——— Bear's Head	68·3
——— Shunnor Fell	68·3
Mean	66·5
Height of Bakestone Edge	1923·2
——— *Pickington Ridge*	1856·7
Do. compared with Ingleborough	1855·9
——— Gt. Whernside	1856·6
——— Shunnor Fell	1855·4
Mean	1856·2

Water Crag below *Ingleborough.*

By obs. at Shunnor Fell	179·9
Height of Ingleborough	2374·6
——— *Water Crag*	2194·7

Water Crag below *Great Whernside.*

By obs. at Shunnor and Great Whernside	116·6
——— Bakestone Edge	116·8
	116·7
Height of Great Whernside	2310·0
——— *Water Crag*	2193·3

Water Crag below *Shunnor Fell.*

By reciprocal observations	158·1
By obs. at Nine Standards	158·4
——— Bakestone Edge	158·7
——— Pickington Ridge	162·3
——— Great Pinch Yate	164·8
Mean	160·5
Height of Shunnor Fell	2351·3
——— *Water Crag*	2190·8

Water Crag above *Bakestone Edge.*

By obs. at Pickington Ridge	267·5
——— Great Pinch Yate	267·9
——— reciprocal	268·8
——— at Shunnor Fell	270·1
Mean	268·6
Height of Bakestone Edge	1923·2
——— *Water Crag*	2191·8

Water Crag above *Pickington Ridge.*

	Feet.
By obs. at Pinch Yate	333·4
——— reciprocal	334·3
——— at Bakestone Edge	335·5
By obs. at Shunnor Fell	338·4
Mean	335·4
Height of Pickington Ridge	1856·2
——— *Water Crag*	2191·6
Do. compared with Ingleborough	2194·7
——— Gt Whernside	2193·3
——— Shunnor Fell	2190·8
——— Bakest. Edge	2191·8
Mean	2191·8

Nine Standards Hill below *Shunnor Fell.*

By reciprocal observations	175·0
By obs. at Bakestone Edge	177·7
——— Pickington Ridge	177·8
Mean	176·8
Height of Shunnor Fell	2351·3
——— *Nine Standards*	2174·5

Nine Standards Hill above *Bakestone Edge.*

By obs. at Bakestone Edge	250·6
——— Pickington Ridge	252·0
——— Shunnor Fell	252·6
Mean	251·7
Height of Bakestone Edge	1923·2
——— *Nine Standards*	2174·9

Nine Standards Hill above *Pickington Ridge.*

By obs. at Pickington Ridge	316·4
——— Bakestone Edge	317·5
——— Shunnor Fell	320·9
Mean	318·3
Height of Pickington Ridge	1856·2
——— *Nine Standards*	2174·5

Nine Standards Hill below *Water Crag.*

By obs. at Pickington Ridge	15·5
——— Nine Standards	16·6
——— Shunnor Fell	17·5
——— Bakestone Edge	18·0
Mean	16·9
Height of Water Crag	2191·8
——— *Nine Standards Hill*	2174·9
Do. compared with Shunnor Fell	2174·5
——— Bakest. Edge	2174·9
——— Pick. Ridge	2174·5
Mean	2174·7

Keasdon below *Shunnor Fell.*

By obs. at Bakestone Edge	712·3
——— reciprocal	712·8
Mean	712·6

Height

	Feet.
Height of Shunnor Fell	2351·3
——— *Keasdon*	1638·7

Keasdon below *Bakestone Edge.*

By obs. at Shunnor Fell	284·5
——— reciprocal	283·9
Mean	284·2
Height of Bakestone Edge	1923·2
——— *Keasdon*	1639·0
Do. compared with Shunnor Fell	1638·7
Mean	1638·8

Great Pinch Yate below *Shunnor Fell.*

By obs. at Water Crag	433·1
——— Pickington Ridge	435·1
——— reciprocal	436·3
Mean	434·8
Height of Shunnor Fell	2351·3
——— *Great Pinch Yate*	1916·5

Great Pinch Yate below *Bakestone Edge.*

By obs. at Pickington Ridge	5·3
——— Water Crag	5·6
——— Great Pinch Yate	5·9
——— Shunnor Fell	6·4
Mean	5·8
Height of Bakestone Edge	1923·2
——— Great Pinch Yate	1917·4

Great Pinch Yate above *Pickington Ridge.*

	Feet.
By reciprocal observations	59·3
By obs. at Shunnor Fell	61·9
——— Water Crag	62·2
Mean	61·1
Height of Pickington Ridge	1856·2
——— *Great Pinch Yate*	1917·3

Great Pinch Yate below *Water Crag.*

By obs. at Pickington Ridge	272·8
——— reciprocal	274·2
——— at Shunnor Fell	276·5
Mean	274·5
Height of Water Crag	2191·8
——— *Pinch Yate*	1917·3
Do. compared with Shunnor Fell	1916·5
——— Bakest. Edge	1917·4
——— Picking. Ridge	1917·3
Mean	1917·1

Height of Harker Fell.

By obs. at Harker Fell	1531·4
——— Shunnor Fell	1530·9
Mean	1531·2

As the errors of measurement at Pickington Ridge are (with one exception) constantly in defect, it would appear that its height had been underrated nearly two feet, or that the actual refraction there had been *less* than the estimated or average quantity. To judge from the subjoined comparison of its height, as deduced from the measurements made at the place itself, and as calculated from observations at other stations, it may be inferred that the latter had been the case.

Height of Pickington Ridge.

	Feet.
By obs. from Bear's Head	1853·9
——— Penhill	1858·2
——— Pinch Yate	1857·5
——— Water Crag	1855·0
——— Bakestone Edge	1856·3
——— Shunnor Fell	1855·4
Arithmetical Mean	1856·1
Rational Mean	1856·3

By the Measurements there of its difference of level with the following stations:

	Feet.
Shunnor Fell	1857·1
Water Crag	1858·0
Bakestone Edge	1858·8
Pinch Yate	1858·0
Nine Standards	1858·3
Arithmetical Mean	1858·0
Rational Mean	1858·1

Difference of the two methods 1·8 feet.

The following Table exhibits the mean heights of the other hills, together with the corresponding errors of measurement committed at the several stations whence they were observed.

Hills.	*Heights.*	Nine Standards.	Water Crag.	Keasdon.	Great Pinch Yate.	Harker Fell.	Pickington Ridge.	Bakestone Edge.	Shunnor Fell.	Ingleborough.	Pen-y-gent.	Whernside.	Knoutberry Hill.	Dod Fell.	Bear's Head.	Settronside.
Whitfield Hill	1346·6	...	...	...	...	...	+3·2	...	...	...	...	...	...	...	−10·7	+5·7
Robincross Hill	1407·7	...	...	...	+0·8	...	−0·4									
Holgate Pasture	1433·4	...	...	...	...	...	0·0									
Calvey	1600·1	...	...	...	+1·0	+1·8	−4·1									
Grinton Grits	1678·8	...	...	...	+3·0	...	−1·2									
Dod End	1683·6	...	...	+0·5	...	...	−2·6									
Satron Hangers	1776·6	...	−1·6	...	+1·1	...	−2·0	+1·0	+1·7							
Gibbon Hill	1781·0	...	...	...	+1·3	...	−0·4									
The Tail Brigg	1799·0	+0·2	...	...	...	...	...	−3·4	+0·9							
The Hoove	1823·0	...	+0·2	...	+1·1	−0·7	−3·1									
Blake Hill	1864·0	...	+2·2	−·04	−0·6	...	−1·4									
E. Stonesdale Moor	1866·2	−0·4	...	+·01	...	...	...	+0·4								
Brownsey	1896·5	...	+1·3	...	0·0	...	−2·1									
Highest Standard top	2152·8	+0·1	−1·8													
The Calf	2220·5	...	...	...	...	...	...	...	+0·4	+2·5	+2·5	−3·0	−0·5			
Rogan's Seat	2207·0	...	−0·2	...	+0·9	...	−2·8	...	+0·7							
Pillar Hill	2260·6	−0·8	...	...	...	...	...	...	−0·3	...	...	...	...	...	+2·7	
Wildboar Fell*	2327·0	...	...	...	...	...	...	...	+0·9	...	−1·1	−4·8	−0·9	+0·9	+3·3	
Hugh Seat	2330·1	+0·5	+3·3	−0·2	...	...	...	+0·2	+2·5	...	−5·9	...	−1·3	+1·0	...	
Mean Error		−0·1	−0·5	0·0	+0·9	+0·5	−1·5	−0·5	+0·7							

* Owing to an error in the allowance for curvature, the difference of level between Bear's Head and Wildboar Fell has been stated at 316·2 feet instead of 311·7. See Phil. Mag. and Annals, N.S., vol. v. p. 435.

The

The true height of (the ground at) Whitfield Hill is without doubt the mean of the measurements, at Pickington Ridge of 1349·8, and at Settronside of 1352·3,—or 1351 feet. The consequent error at Bear's Head, amounting to the unprecedented quantity of 17 feet, appears utterly inexplicable.

Leeds, Feb. 14, 1830. JOHN NIXON.

XXXI. *Narrative of an Excursion to the Summit of the Peak of Teneriffe on the 23rd and 24th of February* 1829. *By* ROBERT EDWARD ALISON, *Esq.*

[Continued from p. 145.]

AT half-past five (thermometer 36°) I awoke my sleepy guide, and in about an hour reached that part of the Peak directly above *Alta Vista Arriba*, called *Mal Pais.* From what I could observe during my two journeys thither, I consider the diagonal line from the base to this point to be rather more than a mile; but a person in ascending has considerably more than that distance to go, from the serpentine direction he is obliged to take in mounting.

Mal Pais consists of large blocks of lava, heaped up together in great confusion, sometimes forming deep hollows, or high and dangerous banks. The lava is of various descriptions, but generally similar to that of the two streams or branches before mentioned; and it appears to have run in a half-fluid state, and to have broken into masses by cooling.

We had made the ascent up to this point by the lights of the bespangled vault above, and we here seated ourselves upon a block of lava to wait for the rising of the sun. In a few minutes a long and bright streak of light orange-colour began to tinge the eastern part of the fleecy clouds below, which was afterwards reflected on the mountains above us. This streak increased in size and intensity, till Aurora with her rosy fingers burst open the gates of the East, and the twinkling lights of heaven immediately hid their diminished heads. The rapid transition from darkness to light was most striking, from the almost total absence of twilight. I was rather surprised at this, as the evening before it was of much longer duration. I suppose this was caused by the great refractive power of the atmosphere, which was afterwards weakened in the morning, by the vapour that was held in solution condensing, and the air becoming dryer.

The ascent over Mal Pais is not so laborious as the track of pumice which we had previously been traversing; as we were

 obliged

obliged to jump from block to block, and to climb over some with the hands and feet. We were again much annoyed by the snow, which was in thick masses between the currents and blocks of lava. The whole of it was frozen hard, forming a surface like glass, which made it extremely difficult to cross, as we were unprovided with proper shoes.

After some exertion we reached a spot called *La Cueva de Nieve* (the Cave of Snow), which is 11·098 feet above the level of the sea, and 2·141 feet above the foot of the Peak. I entered this cave by an aperture near the roof, which is about twelve feet high and eight or nine wide; and was let down to the bottom by means of a rope fastened round my waist. It appears to be formed of large blocks of an earthy and cellular lava, containing large crystals of felspar, which have run together in a half-fluid state, and formed a roof of stalactitic lava, which gradually curves towards the sides. The length in one part, I think, is 120 feet, and about 20 wide. The bottom is filled with water, which is strongly frozen over near the sides, but in the middle it is only covered with a slight skin of ice. The water in some places is ten or twelve feet deep, but in others it is less. At the bottom of it I observed a plant that looked like a species of *Fucus*, but I was not able to get it up for closer inspection. Under the entrance was a wreath of frozen snow and several square blocks of ice, and from the roof hung innumerable icicles, and a quantity of nitrate of potash and a kind of ammoniacal salt. At the further end was an immense bunch of icicles, forming the rough outline of the human figure, which the guides called the Man of Ice: it has been in the same state for many years, which is a proof of the low temperature of that part of the cave which is at a distance from the external air. I am inclined to think that the ice in this cave is produced by the water being impregnated with nitre, and the porous nature of the lava, which conjointly may cool the water down to the freezing point. Water boiled here at 185°, and the bulb of a thermometer, inclosed in silk and wetted with æther, fell 9° in one minute; and a surface of æther, 0·25 of a line in depth, evaporated in the same space of time.

In forty-five minutes after leaving the cave we arrived at a small plain of pumice called the *Rembleta*, situated 11·721 feet above the sea. This plain appears to have been the ancient crater of the Peak, previous to the formation of the present cone, which rises in the middle of this plain to the elevation of 467 feet. The ascent up this cone, or Sugar-loaf as it is called, is the most difficult and laborious part of the journey.

The

The surface is a light pumice and ash, with small pieces of porphyritic lava, covered on the outside with an ochrey crust, forming at the bottom an angle of 35°, and gradually increasing in steepness till near the top; the angle is about 40°, which is nearly the greatest slope the body can ascend without falling backward.

After forty-five minutes of considerable exertion I seated myself on the highest pinnacle of the Peak, 12·188 feet above the sea. Round the summit runs a wall of porphyritic lava, which forms an ellipsis, having the axis from the N.W. to S.E. Within is the crater, which I consider to be about 150 feet long, 100 broad, and 50 deep. I believe it is generally described to be much larger; but having only paced it, I cannot be certain that this is correct. On the east side the wall is broken down, apparently by an ancient eruption of lava, the remains of which appear above the pumice on the ascent: the south-west part has likewise given way, but it rises in a high mass towards the north. The whole of the lavas were in a rapid state of decomposition; and the surface is of a chalky-white colour, produced no doubt by the sulphurous acid gas acting upon the alumina of the lava. The sides and bottom of the crater were rather hot; and from the W.N.E. to the E.N.E. there was a vast number of holes, about an inch in diameter and one or two feet deep, some of them emitting steam, and others sulphureous vapours, which show that they must proceed from different sources, although the apertures were only a few inches apart: the heat of them was considerable, as a thermometer graduated to 133° burst when placed within their influence; and a stick thrust into one of them had the bark completely charred. The steam when condensed was perfectly tasteless, but the apertures whence the other vapours were issuing, were surrounded with the finest needle-shaped crystals of sulphur. In many parts of the bottom of the crater there was a white sort of paste, consisting of silica and alumina; a thermometer placed upon it rose to 107°.

As the plain of the Cañadas, upon which the Peak is situated, rises by degrees to the elevation of nearly 9000 feet, you are not aware of the great height of this volcano till you are on its summit. The clearness of the atmosphere even in the valleys below surpasses that of Italy, and no doubt equals the cloudless sky of most of the Pacific isles; but it is greatly exceeded on the top of the Peak, where on a clear day the eye is able to take in the enormous extent of between five and six thousand square leagues of vision. This peculiar clearness of the atmosphere is probably caused by the great dryness of the air

air over the great African Desert, which is wafted by the east wind to Teneriffe, and the rest of the Canary Islands.

But this extensive prospect astonished more than it pleased me, and I felt unsatisfied from not being able to see some boundary to the horizon. When we first attained the top, the atmosphere below was rather hazy, which in some degree confined the boundary of the horizon; but the islands of Grand Canary, Palma, Gomera, and Hierro were distinctly visible, and we had a complete bird's-eye view of the island below. The fertile plain of Laguna, more than seventeen square miles in extent, with the picturesque valleys, which slope up from the sea on all sides, had the appearance of a narrow belt of verdure round an immense ring of volcanic matter, with the mountain upon which I was placed rising nearly in the centre.

You naturally began to look for the crater whence these fiery torrents issued;—from the position of the Peak it was evident that little, if any, of the devastation proceeded from that source. From a careful observation of the Cañadas, I have no doubt that they were the cause of the surrounding desolation, and that they formed an immense crater to an ancient volcano.

It is possible that volcanic fires had existed in the island in its primitive state, and at last the crust that confined them had burst in its weakest part, which would most probably be in the centre, nearly the present situation of the Cañadas. The form of them is nearly a half-circle, surrounded from the E. by N. to the W.S.W. by an uninterrupted circular chain of mountains, rising like a wall in some places to the height of nearly 1000 feet above the surface. On the north side is a part of the same circular chain, but with a wide chasm, which separates it from the other: this part is called the *Risco de la Fortaleza* and *El Cavison.* From the outer part of this chain spring several high ridges of mountains like buttresses to it, forming fertile valleys between;—to make use of a familar illustration, the Cañadas may be likened to a half-wheel, the nave being the Cañadas, and the spokes the ridges of mountains diverging from it. From that part of the chain called La Fortaleza and El Cavison, springs a high ridge, the upper part of which is called Tigayga, which running to the sea forms the western boundary of the valley of Orotava. From the east side of the unbroken chain runs another high ridge, called *Pedrogil*, *La Florida*, and *La Resbala*, which form the eastern limit of the same valley. On the south-east is another ridge, which is divided into several branches, and on the south is part of one which

no

no doubt belonged to the main body. Towards the W.S.W. the foot of the Peak is much lower than on the sides next the Cañadas, and the surface is totally different; it likewise has no circular chain of mountains, nor any remains, that I could observe, of ridges of mountains springing from them.

I think the Cañadas formerly were circular, and considerably higher than they are now, but that they have sunk from the immense quantity of matter they have thrown out; and it is very probable that the W.S.W. side was surrounded by a similar chain to that on the opposite point, but it was possibly destroyed by some violent convulsion of nature at the same period that the chasms were formed in the present circular chain round the Cañadas.

Appearances strongly indicate that the whole island at one period gradually sloped on all sides from the Cañadas to the sea, and that the beautiful valleys of Orotava and Icod were formed by a sinking of the strata in the centre of them. What makes this apparent is, that the mountains called Pedrogil, La Florida, and La Resbala, which form the eastern boundary of the valley of Orotava, are nearly on the same level and inclination as the opposite mountains of Tigayga, and *Icod-el-Alto*, which form the western limit: as the upper part of the sides next the valley are almost perpendicular, or, what is practically the same thing, form an angle in some places of 60 or 70 degrees, and as they are covered with a luxuriant vegetation, I was not able to discover whether the strata of both ridges have the same succession.

Several minor volcanos are scattered in different parts of the Cañadas, which from the top of the Peak look like so many large hillocks on a sandy plain; the craters of two of them are very distinct, and the side which discharged the lava is evident. The surface of this immense crater is dotted over with masses of lava, which at a certain distance from the Peak is of a *grünsteinic* character; some are only a few inches above the surface, and others are several yards: at first sight they appeared to be without order, but upon closer inspection I found that they were generally disposed in concentric circles. It is difficult to account in a satisfactory manner for this appearance, unless they were formed after the general sinking of the Cañadas, when fresh eruptions breaking out in the centre formed new craters, which fell in one after another, and were ultimately covered over with a thick bed of pumice thrown out by the Peak or some of the numerous volcanos on the surface.

Besides these before-mentioned masses of lava there were immense blocks upon the surface of the pumice, some of them 12 feet high and nearly 40 in circumference; some of them were

were porphyritic, with large crystals of felspar, and others looked like pitchstone, with a mixture of pumice; but from its not breaking in a conchoidal form, I am inclined to think it is an obsidian that has suddenly cooled.

[To be continued.]

XXXII. *On the Dying Struggle of the Dichotomous System. By* W. S. MacLeay, *Esq. M.A. F.L.S. In a Letter to* N. A. Vigors, *Esq. F.R.S.*

[Concluded from p. 140.]

DR. FLEMING's late work on British Animals I repeat that I have never seen; but his series of affinity as given in the "Philosophy of Zoology" is as follows:

Vertebrata.
Cephalopoda.
Mollusca.
Tunicata.
Annulosa.
Cirripeda.
Annelida.
Entozoa.
Radiata.
Acrita.

In the *Horæ Entomologicæ*, however, I have not merely said, but I have proved to demonstration, that by the admission of the *Mollusca* to a higher rank than the *Annulosa*, the latter would be so far separated from the *Vertebrata* as to occasion an unnatural interruption of the series. Dr. Fleming now says that the degradation of the *Mollusca* is decreed by me, "*although not very logically*," in these terms; "It follows therefore that though they undoubtedly possess a very complete system of respiration and circulation, the *Mollusca* are inferior in the scale of nature to the *Annulosa*." These are indeed my words; but the Doctor took good care not to cite the passage which they follow, because that would have proved the deduction to be strictly logical. He appears to fancy, that, on account of their system of respiration and circulation, the *Mollusca* are superior to the *Annulosa*; in other words, that an ascidia and an oyster are superior animals to the bee and the ant! But *Annulosa* breathe as well or better than any *Mollusca*, and in my present opinion, founded on late dissections, possess a circulation, although of a most peculiar kind. But if they had no system of circulation whatever, the place assigned by me to the bee would not be altered, as Dr. Fleming would have

himself

himself seen if he had not mistaken Lamarck's "rapports entre des parties semblables ou analogues" for relations of analogy. Lamarck says in the very passage, "Voici l'ordre d'importance qu'il faut attribuer aux organes particuliers que la nature a employé dans l'organization intérieure des animaux:

"1. Les organes de la digestion.
"2. Ceux de la respiration.
"3. Ceux du mouvement.
"4. Ceux de la génération.
"5. Ceux de sentiment.
"6. Ceux de la circulation."

Hist. Nat. des Anim. sans Vert., vol. i. p. 360.

Now I say, admitting that the organs of circulation in *Annulosa* may be, or are, inferior to those of *Mollusca*, still in the above five other systems of superior importance the former animals are infinitely more perfect in their organization than the latter. So much for Dr. Fleming's judgement of logic; a judgement which enables him also to decide that by thus placing the *Mollusca* below the *Annulosa*, I have *arbitrarily* arranged the animal kingdom into the following five groups:

Acrita.
Mollusca.
Vertebrata.
Annulosa.
Radiata.

I must persist however in asserting that neither the arrangement of these groups, nor the groups themselves, are arbitrary. Both I may say are almost mathematically proved to be natural, since the five groups are distinguished, as I have shown (*Hor. Entom.* p. 200,) by their five respective nervous systems; and because, as to their arrangement, it certainly requires the possession of remarkable powers of thought to entitle us to suspect that a bee is an inferior animal in nature's scale to an oyster.

The only objections that the critic dares to adduce against the above arrangement of the animal kingdom are two, viz. the relation of affinity between cuttle-fish and chelonial reptiles on the one hand, and between fishes and red-blooded worms on the other. This is indeed scarcely the place for entering upon so vast and pleasing a field of anatomical demonstration; my letter is already too long, but I trust notwithstanding that you, my dear friend, will excuse my trespassing still further on your time by a few remarks on this part of the subject.

There is a certain rule in nature so evident that I never knew it doubted, except by Mr. Bicheno, who tells us that no

groups exist in nature but genera and species; thus differing *toto cælo* from Dr. Fleming, who by advocating the binary system goes to the other extreme, and states that there is an infinite number of natural groups superior to genus and species. The rule alluded to is, that groups of different degree vary in their distance from each other. It is manifest, for example, in the animal kingdom, that the distinction between two congeneric species does not depend on so important a point of structure as the distinction between two contiguous genera, nor this again on so important a point as the distinction between families; and, to proceed in like manner on, the distinction between two sub-kingdoms like *Vertebrata* and *Mollusca*, must be greater than all, because it depends on some most important point of structure. A species is not only nearer to its congener than *Vertebrata* are to *Mollusca;* but there exists, although it may be impossible to calculate its exact value, a manifest gradation of intervals between the various two contiguous groups of the same rank, which are intermediate between a species and sub-kingdom. A vertebrated animal, for instance, is marked out not merely by its vertebrated structure; the very circumstance of not being vertebral argues other most important distinctions in the general structure of the animal. A gap therefore occurs between *Vertebrata* and all the rest of the animal kingdom. Not only Lamarck but all other naturalists have admitted it. How then is there no continuity here? I shall show that there is, and again I say an *hiatus* is not a *saltus*. Continuity in gradation of structure cannot exist, as we have seen, without intervals; and the size of these intervals does not lessen the truth of the chain, because some of the links may not yet be discovered. How then, you ask, am I to prove that the chain is continuous? I answer, Simply by ascertaining which animals of one group come the nearest to those of the other. If there be no approximation, if all the animals remain *equally* distant, then there is no continuity; but if one animal of the one group approaches to the structure of the other, then there is a chain of continuity possessing indeed only one link, but not the less presenting a mode of transition from one form to the other. Thus if the only animal existing between Mammalia and Fishes were one Penguin, it would still be in the path of passage. But if a Tortoise existed in addition, the chain would be more complete, and if one Frog existed also, the chain would scarcely escape notice. In it there is a regular and obvious gradation of structure, although the chasms remain vast. Nevertheless there is no *saltus* or leap by nature over one form to another; and Linnæus knew this, although by placing a whale imme-

diately

diately between the cow and the hawk, he took himself a leap as frolicsome as Dr. Fleming has since taken in placing the *Tunicata* between the *Mollusca* and *Annulosa*, as the link of their immediate connection.

Such being the unequal nature of chasms, and their existence being necessary to form that of a continuous chain of animals, the question now to ask is not whether the transition from *Vertebrata* to the *Invertebrata* of Lamarck be imperceptibly gradual (for that, although theoretically supposable, has been shown in nature to be impossible), but whether any animals of the *Mollusca* approach more than the others to the structure of the *Vertebrata*, and any of the *Vertebrata* more than the others to that of the *Annulosa*. I have already decided this last question, and proved a number of relations of affinity between *Mollusca* and *Vertebrata*, and between *Vertebrata* and *Annulosa*, which cannot, as the Doctor imagines, be relations of analogy, for they want the great characteristic of these last, namely, their parallelism. If *any* relations exist between *Cephalopoda* and Chelonial reptiles, they cannot be relations of analogy, and therefore must be relations of affinity. Dr. Fleming indeed gives, as usual, a garbled quotation from a passage that I have cited at length from M. Cuvier, but I will not repeat here what I have sufficiently proved. So far as the mere name goes, the genus *Chelys* is cephalopod as well as *Loligo*. I trust that he will endeavour to become acquainted with these and other genera of the two groups, and that he will compare Messrs. Guthrie's and Holberton's late anatomy of tortoises with that of *Cephalopoda* by M. Cuvier, or, still better, that he will at length take the scalpel in hand. The genera just mentioned are not indeed the nearest to each other of their respective groups, but enough and more than enough is evident to prove their approximation. If *any* relations exist between them, as there is no parallelism they cannot be relations of analogy; or even could Dr. F. reduce the numerous relations I have pointed out (*Horæ Entom.* p. 248—261) to parallelism, and thus show them to be relations of analogy, it would only add to our conviction that the two groups are contiguous, for such parallelism characterizes two contiguous circles. So, let Dr. Fleming dichotomize until doomsday,—*unless indeed he denies the truth of the anatomical facts I have detailed on this question*,—the goal he will infallibly arrive at will be the same, namely that *Vertebrata* are connected with the *Mollusca* by means of the *Cephalopoda* and Chelonian reptiles. But "has the attempt to unite the *Vertebrata* with the *Annulosa* been more successful?" No, not more so; but quite as successful.

 Our

Our critic talks a great deal about lampreys and leeches, about the former having eyes and the latter none, with many more observations of similar novelty and to much the same purpose. He must excuse me, but I really have neither time nor inclination to notice them*. I only entreat him to recollect that the transition from *Vertebrata* to *Annelida* is not made directly by the *eyed* lamprey, but by a *blind* animal called *Myxine glutinosa*†, of which the last naturalist that has described it says, "Voisins des *Lamproies* par les *Ammocætes*, auxquels ils ressemblent beaucoup, et avec lesquels ils forment *un passage très naturel de la classe des Poissons à celle des Annelides*." The whole article, which is by Bory de St. Vincent, in the *Dictionnaire Classique d'Histoire Naturelle*, deserves to be read, as it details the grounds of the above affinity. Cuvier had already pointed out the same self-evident fact; yet here is a writer saying "that he is astonished that any individual who ever made the comparison should have been able to perceive very evident affinities where there existed only a few very remote and insignificant analogies." I repeat it, and I have proved it, that he does not know an affinity from an analogy.

Now the foregoing demonstrated affinities are his chosen points of attack, his fancied mortal stabs. *Valeant quantum valere possint*, and I find myself still safe on my legs. To be sure the tender-hearted gentleman knows more vulnerable points, but he thinks it is useless to be cruel. "In connecting the other circles, defects equally remarkable and extensive might be pointed out, were it necessary to enlarge." Equally remarkable and equally extensive I trust they would be—I want no better.

He says that the quinarian circle, like the tripod of the Isle of Man, "stabit quocunque jeceris." So far as relates to the stability of the system, no doubt the remark is perfectly correct; but the Doctor is I suspect here awkwardly alluding

* I suspect, however, that Sir Everard Home will scarcely feel obliged to him for citing his authority on a point which has long since been disproved by the celebrated French anatomists MM. Majendie and Desmoulins. His admiration of Sir Everard's discovery of the hermaphroditisms of the lamprey, induced him to institute the genus *Homea* in his honour; although it ought clearly to be called *Gastrobranchus*, the original fish described preserving the generic name *Myxine* given to it by Linnæus. In this way Bloch's very appropriate name would not be lost. It is truly unfortunate that the above denial of any affinity between *Vertebrata* and *Annulosa* not only proves our Doctor never to have seen his "*Homea*," which he only knows by Sir Everard's description, but that even to this day he remains in ignorance of the most important point of its structure.

† See *Horæ Entomologicæ*, p. 203.

to

to its facility, on which head I can only say that when this worthy has produced to the world a natural group differently divided into five circles, each two connected by parallel analogies, then I will believe him. To do it once, the *once of nature* is abundantly difficult, and the proof is the slowness with which we are evolving the system.

Only two objections of Dr. Fleming's remain to be considered. The first of these is, that "one set of organs are employed to establish a connection here, and another to establish a connection there." And why not? The white man and the negro are distinguished by their feet; while the Asiatic and African elephants are known from other animals by their trunk. I admit therefore the fact, and yet our system when completed will express the variation of all organs. Will it be believed that I am perhaps one of the first after Lamarck who attempted to prevent the selecting such systems of organs at hazard, and perhaps the very first who prescribed the following rule for their choice? "Like every other solitary character that can possibly be adopted for the groundwork of a zoological system, the mode of generation ought to rise in importance only in inverse proportion to its degree of variation. In a group of animals for instance, where it varies according to the species, it is manifestly of less importance as affording natural characters, than among those groups where it remains less subject to variation." These words prove that although we neglect no characters, yet we have a rule for deciding upon their respective values.

The other objection is stated as follows: "The same means which the quinarians employ to divide a group when a fifth is wanting might enable them to subdivide others." Now the same means will *not* enable them to subdivide others; for they have remarked in nature that every natural group returns into itself, in other words is a circle, and no group presents to their view more than five such groups of equal degree returning into themselves. Again the Doctor destroys himself by an example. "What," says he, "but the most obvious prejudices could induce Mr. MacLeay to divide *Insecta* into *Mandibulata* and *Haustellata*, and *leave entire the Arachnidæ?*" He affords the answer himself, but I scorn to take advantage of his ignorance. He means *Arachnida* *, and I will now answer his question. *Mandibulata* and *Haustellata* do not present one circle, but

* Let Dr. Fleming visit the Collections of the Metropolis; there he will see a variety of forms of *Arachnida*; or still better, for *Arachnida* are not easily preserved, let him visit the Torrid Zone, the metropolis of *Arachnida*, and he will see a continuity of forms as remarkable as in *Mandibulata* or *Haustellata*. It is only a week since I discovered a spider with only two eyes and every day produces something new in this class.

two,

two, therefore they form two natural groups. "Omnis sectio naturalis," says Fries, "circulum per se clausum exhibet." And as the *Arachnida*, on the other hand, present not two circles but one, therefore they compose one natural group *.

This is the most common shoal upon which naturalists run aground. They forget the necessary and obvious inequality of chasms in different natural groups, and found their own artificial groups upon this most fluctuating of all principles, of which moreover they can never detect, by a mathematical comparison of intervals, the exact value. Instead of this conduct, they ought to look to the only certain principle, the closure of the group by the series having returned into itself. This is the only test of a natural group. From the above expression of Dr. Fleming, one would really think that I had never divided *Arachnida*, but this is not meant. I certainly have not left it entire, and may possibly have divided it just as he would; and the Doctor merely means, that I have not given the divisions such dignity as he judges, from certain chasms, that they deserve. My object, however, was to express every natural group, and above all to demonstrate the chain of continuity. All the rest, such as the comparative width of fluctuating chasms, is but leather and prunella.

By the by, if much more profound naturalists would attend to the above definition of a natural group, they would not so often flounder about in all the difficulties which necessarily attend the supposition of two determinate numbers. We should not hear them agreeing with Mr. MacLeay, that in most cases five is the natural number, although in some cases there may be as many as seven. Were they to put their seven groups each to the test of returning into itself, they would find that in fact they compose only five natural ones. The determination of the particular number must no doubt depend on observation; but I have already proved in another place, that, whatever this observed number may be, there is necessarily only one. The other parts of the quinary system, such as the maxim of variation, the distinction of relations of affinity from those of analogy, and the progression of relations of affinity in circles, are all truths depending upon each other, as I have shown in the Linnæan Transactions. Grant one, and you arrive at the other.

* If Dr. Fleming understood what he writes about, he would have known that two out of five groups always come nearer to each other than the other three aberrant groups; that Aristotle gave the name of *Ptilota* to the normal group or centrum in the present case; and that, to use the words of M. Fries, "*Centrum abit semper in duas series*," which here are *Mandibulata* and *Haustellata*.

Dr

Dr. Fleming gives just and due praise to the great naturalists of England, Ray, Willoughby, and Lister, and most properly exalts them above Linnæus; but it is evident, from the mode in which he characterizes their works in contradistinction to those of Linnæus, that he never read them, and consequently that he is, in celebrating them, only a plagiarist from some person who has called his attention to their merits. Let him say who this person is.

If any one thing be more clearly prescribed in Natural History than another, it is perhaps a species. Linnæus has defined species thus: "Species tot sunt quot diversas formas ab initio produxit infinitum Ens; quæ formæ, secundum generationis inditas leges, produxere plures, at sibi semper similes." Our establisher of fixed principles, however, scorns this definition, and favours us with the following new one: "When the similarity between objects is *complete*, the individuals, however numerous, are regarded by the naturalist as constituting a species." This is certainly a shorter definition than the former; and to say nothing of the difference of the sexes, as the similarity between two objects is never *complete*, it has the advantage of making as many species as there are individual beings.

His mode of distinguishing the sexes is equally accurate. "In the more perfect animals we shall find that one individual of the species will be drawn by an *immaterial tie*"—"to unite with another individual in the propagation of its kind. If this selection has taken place in a state of freedom, we may with certainty infer that the two individuals are male and female." Provided this attraction be the certain mode of discovering the sexes, the Doctor may perhaps, on his next spring visit to the ditch that surrounds his glebe, discover a carp or tench to be the female of a frog. As the fact to which I allude is not rare, to be convinced of the affinity, he will, without much difficulty, have the evidence of observation. Insects, I suppose, he considers as less perfect animals, and so falling out of the scope of his definition; else would singular males and females be discovered.

But there is no end to the absurdities of this article, which so singularly ornaments the Quarterly Review. I might run on and investigate his "caterpillar's suckers," his "albuminous skeletons," and his "bisulcated pigs," &c. &c.; but I will not, and only pray that the dust I have raised about his ears may lie light on these as well as all his other vagaries.

Believe me ever, my dear Vigors, most truly yours,

Havana, March 1, 1830. W. S. MacLeay.

Notes

XXXIII. *Notes on the Geographical Distribution of Organic Remains contained in the Oolitic Series of the Great London and Paris Basin, and in the same Series of the South of France. By* Henry T. De la Beche, *F.R.S. &c.*

[Concluded from p. 44.]

Annulosa.

1. Vermicularia compressa (*Y. & B.*). *Coral oolite, and inferior oolite.* Yorks. (Phil.).
2. ——— nodus (Phil.). *Cornbrash and great oolite.* Yorks. (Phil.).
1. Serpula squamosa (*Bean*). *Coral oolite.* Yorks. (Phil.).
2. ——— lacerata (*Phil.*). *Calc. grit, and great oolite.* Yorks. (Phil.).
3. ——— intestinalis (*Phil.*). *Oxford clay and cornbrash.* Yorks. (Phil.).
4. ——— deplexa (*Bean*). *Inf. oolite.* Yorks. (Phil.).
5. ——— capita (*Phil.*). *Lias.* Yorks. (Phil.).
6. ——— triquetra. *Inf. oolite.* Mid. and S. Engl. (Conyb.).
7. ——— quadrangularis. *Oxford clay.* Normandy (Desn.).
8. ——— sulcata (*Sow.*). *Calc. grit.* Oxford (Sow.).
9. ——— tricarinata (*Sow.*) *Calc. grit.* Oxford. *Coral rag.* Steeple Ashton, Wilts. (Sow.).
10. ——— triangulata (*Sow.*). *Bradf. clay or great oolite.* Bradford (Sow.).
11. ——— runcinata (*Sow.*). *Coral rag.* Oxford (Sow.).

——— species undetermined. *Coral rag, Oxford clay, cornb., forest marb., Brad. clay, great oolite.* Mid. and S. Engl. (Conyb.). *Brad. clay.*

1. Dentalium giganteum (*Phil.*). *Lias.* Yorks. (Phil.).
2. ——— cylindricum (*Sow.*). *Lias.* Mid. and S. Engl. (Conyb.).

Dentalium, species undetermined. *Calc. grit.* Yorks. (Phil.).

Summary of the foregoing List.

Mammalia: Didelphys, 1 species. Reptilia: Pterodactylus, 1 species, and probably another; Crocodilidæ, 5 species, and probably others; Megalosaurus, 1; Geosaurus, 1; Plesiosaurus, 5, one being questionable; Ichthyosaurus, 4, and probably others; Testudinata, species undetermined. Insecta: Coleopterous. Pisces: 1 species, and many others not determined; Ichthyodorulites, or fish spears; palates, teeth, &c. Crustacea: Astacus 1; many other crustaceous remains undetermined. Zoophyta: Spongia, 1, and probably others; Alcyonium, species not determined; Turbinolia, 1, and others not determined; Turbinolopsis, 1; Entalophora, 1; Limnorea, 1; Caryophyllia, 7, and others; Millepora, 6; Favosites, species not determined; Astrea, 6; Cellepora, species not determined; Fungia, 1, and probably others; Spiropora, 4; Eunomia, 1, and probably others; Cyclolites, 1; Chrysaora, 2; Theonoa, 1; Idmonea, 1; Alecto, 1, and probably others; Berenicea, 1; Terebellaria, 2; Retipora? Cellaria, 1; Thamnasteria, 1; Explanaria, 1; Madrepora, Meandrina,

drina, and Eschara, species not determined; remains of other Polypifers, genera doubtful.

RADIARIA.

Cidaris, 9; Echinus, 1, and probably others; Clypeus, 6; Spatangus, 1; Clypeaster, 1; Galerites, 2; Ananchites, 1; Nucleolites, 2; and numerous Echinital remains of undetermined genera; Asteria, species not determined; Ophiura, 1; Apiocrinites, 2; Pentacrinites, 6; and Crinoidal remains, genera not stated.

CONCHIFERA AND MOLLUSCA.

Pholas, 2, one questionable; Pholadomya, 16; Panopæa, 2; Mya, 7; Sanguinolaria, 2; Crassina, 7; Amphidesma, 5; Lutraria, 1; Gastrochæna, 1; Psammobia, 1; Lucina, 3; Unio, 7; Pullastra, 2; Venus, species not stated; Cytherea, 1; Corbis, 2; Tellina, 1; Astarte, 12; Corbula, 4, one questionable; Cardium, 11; Isocardia, 6; Cardita, 3; Trigonia, 12; Hippopodium, 1; Nucula, 9; Cucullæa, 13; Arca, 3; Pectunculus, 2; Crenatula, 1; Inoceramus, 1; Modiola, 16; Mytilus, 5; Trigonellites, 2; Mactra, 1; Pinna, 7; Perna, 1; Gervillia, 7; Avicula, 11; Plagiostoma, 17; Pecten, 20; Lima, 4; Exogyra, 1; Chama, 2; Plicatula, 1; Ostrea, 23; Gryphæa, 14; Lingula, 1; Terebratula, 45; Spirifer, 1; Donacites, 1; Cyclas and Lithodomus, species undetermined; Orbicula, 3, one questionable; Delphinula, species not stated; Natica, 5; Turbo, 9; Trochus, 21; Turritella, 4; Myoconcha, 1; Terebra, 4; Ancilla, species undetermined; Emarginula, 1; Patella, 6; Rissoa, 4; Melania, 4; Bulla, 1; Murex, 1; Cirrus, 4; Actæon, 5; Nerinea, species undetermined; Pteroceras, 3; Rostellaria, 4; Phasianella, 1; Solarium, 2; Nerita, 4; Buccinum, 1; Auricula, 1; Planorbis, 1; Helicina, 4; Tornatella and Ampullaria, species undetermined; Belemnites, 12, and most probably others; Orthoceras, 1; Turrilites, 1; Nautilus, 10, and probably others; Ammonites, 114.

ANNULOSA.

Vermicularia, 2; Serpula, 11; Dentalium, 2.

VEGETABLE REMAINS.

Fucoides, 1 species; Equisetum, 1; Pachypteris, 2; Pecopteris, 6; Sphænopteris, 5; Tæniopteris, 2; Pterophyllum, 1; Zamia, 11; Zamites, 4; Thuytes, 4; Taxites, 1.

Upon a review of the above lists, it will, I think, be observed that our knowledge of the vegetable remains is too limited to enable us to form any general conclusions respecting them. Mammalia have been only observed in one locality, Stonesfield.

 Of

Of Pterodactyles our knowledge is limited. Crocodiles seem to have existed during the whole deposit of the oolite, and to have been widely distributed; the same may be said of the Ichthyosaurus and the Plesiosaurus. Neither Pterodactyles, Crocodiles, Ichthyosauri, nor Plesiosauri, have yet been observed in the South of France. The Geosaurus has at present only been noticed in the lias of Wurtemberg, and the Megalosaurus in the Stonesfield slate, near Oxford, and in the great oolite of Normandy*. Respecting Tortoises, Turtles, and Fish, we do not possess information that can lead to any useful conclusions. Insects are yet known only in the oolite at Stonesfield. Polypifers occur in considerable abundance in particular places, and, as it would appear, principally in the oolite that has been thence named Coral rag, and in the upper part of the great oolite, which has thence obtained the name of Calcaire à Polypiers in Normandy. It has been imagined that the coral rag is a constant rock in the oolitic series; which is supposing that during the deposition of the oolite there was a time when the whole bottom of the sea was covered by an universal coral reef, and that the same polypifers could exist under various pressures of water; suppositions that are at variance with the habits of existing polypifers. When polypifers do however occur in any abundance, they have been observed in the strata above noticed, in both cases accompanied by remains of the genera Clypeus and Cidaris. By reference to the above lists, it will be also observed that several shells are common to the coral rag and great oolite. The Crinoidal remains contained in the oolite appear principally Pentacrinites and Apiocrinites; the former occurring abundantly and widely distributed in the lias; the latter in the great oolite, or its accompanying beds, the cornbrash, forest marble, or Bradford clay.

Of the Conchiferous and Molluscous remains entombed in the oolite, 540 species have, according to the foregoing list, been determined; of these 114 (more than one-fifth) are Ammonites, which are not only abundant as species, but as individuals, so that some beds are almost wholly composed of them. The great abundance of Ammonites and Belemnites may be stated as a great characteristic of the oolitic series: they are particularly numerous in the lias.

As far as our knowledge of the organic characters of European rocks at present extends, the shells contained in the ooli-

* Dr. Buckland informs me that in the year 1826 he recognised many bones of the Megalosaurus in the museum of Besançon from the oolite of that neighbourhood.

tic

tic series, though frequently not confined to particular portions of that series, even in England and France, still seem as a mass characteristic of it within the limits noticed in this memoir. The following shells, according to the foregoing list, contained in the oolite, have been noticed in the chalk and green sand:

1. Terebratula subrotunda. *Chalk.* Sussex (Mantell).
2. ——— carnea. *Chalk.* Suss. (Mant.). *Chalk.* Paris and Normandy (Al. Brong.).
3. ——— ovata. *Chalk and green sand.* Suss. (Mant.).
4. ——— biplicata. *Green sand.* Suss. (Mant.).
5. ——— lata. *Green sand.* Suss. (Mant.).
6. ——— ornithocephala. *Green sand.* Perte du Rhone, Fis (Al. Brong.).

1. Gervillia aviculoides. *Green sand.* Suss. (Mant.).
2. ——— acuta. *Green sand.* Suss. (Mant.).

1. Cucullæ decussata. *Chalk marl.* Suss. (Mant.). *Chalk marl.* Rouen (Al. Brong.).

1. Turbo rotundatus. *Green sand.* Blackdown (Sow.).

1. Rostellaria Parkinsoni. R. Sowerbii (*Mant.*). *Green sand.* Blackdown. (Sow.). Suss. (Mant.).

1. Ammonites splendens. *Gault.* Suss. (Mant.)
2. ——— lævigatus. *Gault.* Suss. (Mant.).

1. Cirrus depressus. *Chalk.* Suss. (Mant.).

1. Exogyra digitata. *Green sand.* Lyme Regis, abundant (De la B.).

1. Mya mandibulata*. *Green sand.* Devizes and Lyme Regis.

Whether we are to conclude that the same species occur in the cretaceous and oolitic groups depends on the credit we may consider due to the respective authors cited; in fact, to their ability in determining specific differences;—no easy task, but I do not see why, with our present limited knowledge, we should determine the question without further examination.

It has been generally supposed that the rocks of the oolitic series have been deposited in a sea; and the great abundance and proportion of marine organic remains entombed in them would seem to render this supposition probable. We have no data by which to form any conception of the extent of such a sea. The portion of the world occupied by the oolitic rocks, noticed in the foregoing remarks, is of insignificant extent, comprised within a few degrees of latitude and longitude. How far the oolitic series may hereafter be found to extend, it would be difficult to say. It is possible that it may merge in some other great rock deposit, or even be considerably developed at the expense of the cretaceous rocks above or the red sandstone rocks beneath.

It will be remarked that terrestrial plants, lignite, or coal, occur more or less throughout the whole series. May we not

* Supposed to be Mya mandibula of Sowerby.

 therefore

therefore conclude that dry land existed in, or not far distant from, the comparatively small space containing the organic remains enumerated in the foregoing table? The principal deposits of carbonaceous matter would seem to be Brora and Yorkshire.

Most of the great Saurians now buried in the oolite would seem also to have required the protection of land; for though the Ichthyosauri might, like the Porpesse, brave the waves of an ocean, the structure of the Plesiosauri would seem to unfit them for such exposure; and, judging from the habits of modern crocodiles, the ancient species are not likely, from choice, to have quitted the neighbourhood of land. The Pterodactyles probably flew over the land, and the Didelphys must have lived upon it.

The quantity of corals contained in the forest marble, great oolite, or coral rag, would seem to show that the places, where these remains are the most abundant, must have had a *comparatively* shallow covering of water at the time these Zoophytes existed.

The evidence, therefore, would seem to be in favour of a comparatively shallow sea, interspersed with dry land, for the formation of so much of the rocks usually termed oolitic, as occur within the space treated of in these notes. The great abundance of Oysters and Gryphites may probably also be in favour of comparatively shallow water*.

I have been thus particular in enumerating what may appear to be evidence of comparatively shallow seas, because the same deposit may have been, and probably was, going on in contiguous and deeper portions of the ocean, and because these continuous portions of the same formation may differ very considerably both in mineralogical and organic character. Thus the oolitic series of England and of France may be represented even so near as Italy and Greece, by a series of beds so different in organic contents, as at first sight to be considered distinct.

Endeavours to trace the small divisions into which the oolitic series of England has been separated, are no doubt useful if our attention be directed to an examination of the areas over which certain minor causes may have operated: but when these small

* It is remarkable that the three great argillaceous deposits of the oolitic series contain an abundance of either Gryphites or Oysters, and that the Saurian remains are most commonly observed in the same strata. Thus the Ostrea deltoidea in England, and the Gryphæa virgula in France, have been termed characteristic of the Kimmeridge clay; the Gryphæa dilatata is a common shell in the Oxford clay; and the Gryphæa incurva *Sow.* (G. arcuata *Lam.*) is abundant in the lias.

divisions

divisions are supposed to be general, and geologists expect to meet with cornbrash in China, much mischief ensues, and much valuable time and talent is wasted in endeavours to prove that the whole surface of the world is minutely the same as any given quarry, province, or even kingdom.

It does not seem irrational to infer that such minute divisions, characterised by peculiar fossils, can only be traced over comparatively small areas, unless we are to suppose that the same animals and vegetables existed over the whole surface of the globe at the same time,—that these were suddenly destroyed—imbedded 10, 20, 100, or 400 feet deep, as the case might be,—that then there was a new creation,—then a total destruction, and so on.

So far as respects the limited area of which we have been treating, there do appear to have been certain general or nearly similar causes in operation. Consequently, though many species of shells &c. are not strictly confined within the small limits usually assigned them, still in the oolitic series taken as a mass there would appear to be a general resemblance in organic character.

Belemnites seem to have been equally abundant in the lower parts of the series everywhere. The Gryphæa incurva is found under similar circumstances in Scotland, England, and France; and the same may be said of many other shells. Ichthyosauri of the same species occur in similar situations in Germany, England, and the North of France.

It therefore would appear that, during the deposit of the oolitic series, the geographical distribution of the animals, whose remains we now find entombed in its various beds, was not widely different throughout the area treated of in these Notes. It also would appear, although some animals may have existed in one place and not in another, and although these remains may occur in various beds in one locality and be confined to one bed in another, that the organic character of the mass is similar in Scotland, England, and France.

XXXIV. *On the Œconomy of the Steam-Engine, and on some very general Mistakes regarding the Expansions of* MM. Dulong *and* Petit. *By* Henry Meikle, *Esq.*

To Richard Taylor, Esq.

Sir,

FROM looking into the Quarterly Journal of Science for January—March, 1830, page 186, I observe that Mr. Ainger, who some time ago showed such a laudable zeal in claiming

claiming for our countrymen the invention of whatever is valuable in the steam-engine, is no less industrious in bringing forward the important discovery that steam is preferable to other vapours in an œconomical point of view. By turning to the Philosophical Magazine for July 1826, page 41, it will be seen that if this idea be still new to Mr. Ainger, who is uncommonly well versed in every *minutia* connected with the history of the steam-engine, it is by no means new to the public; particularly, so far as regards the vapour of alcohol, which next after steam had been so much oftener proposed than any other, that the consideration of its case was quite sufficient in starting the subject: the same mode of comparison being so obviously applicable to other vapours.

I am, however, very much at a loss to see the need of such a complicated calculation as Mr. Ainger has employed. The method which I followed is incomparably more simple. It is evident that if we know what rises can be produced in the temperature of a certain mass of cold water, by the condensations of known quantities of different vapours having equal tensions, we have the main *datum* for making the comparison, so far as regards the expense of heat. The rest is too obvious to require any remark.

It will be found that some of the numbers on page 41, line 27, have not been correctly copied from Dr. Ure; as I had put down 42° and 49°·5 in place of 42°·5 and 49°. Fortunately this oversight has scarcely any influence on the result.

MM. Dulong and Petit, in stating their results, place in one column a series of temperatures; and opposite to these, in another column, the dilatation. This undefined *dilatation* is understood by some to signify the expansion for one degree at the temperature opposite; by others, the mean expansion for one degree of the interval between the temperature opposite and the temperature next before it; both of which senses are generally far from what the authors meant. Whoever takes the trouble to examine thoroughly the memoir of Dulong and Petit, will find that they reckon all their intervals from the freezing point up to the temperature opposite the dilatation; and that such *dilatation* is the mean for one degree of the whole interval. The quotations from Dulong and Petit in most of our English authors are tainted with these mistakes; but as it is some time since I attended to this, I can only mention at present the quotations from Dulong and Petit in those very useful works, Dr. Ure's Dictionary of Chemistry, and the Library of Useful Knowledge; many of which will be found very incorrect, particularly where they state the expansions of

of bodies for intervals of temperature which do not commence at the freezing point; for MM. Dulong and Petit, in their memoir *Ann. de Chim. et Phys.* tome vii., from which the quotations are meant to be taken, have not given the expansions for such intervals; but when the true numbers are computed for these intervals from the other results of Dulong and Petit, they differ materially from those given for them in our English quotations. I am, Sir,

Your very obedient servant,

HENRY MEIKLE.

XXXV. *Letter from the* Rev. W. D. Conybeare, *M.A. F.R.S. F.G.S. &c. on* Mr. Lyell's "*Principles of Geology.*"

To Richard Taylor, Esq.

Mr. Editor,

I HAVE just received from my bookseller a geological work, which appears to me (while yet I am in very many points compelled to hesitate, if not to differ, as to its conclusions) of the first merit and importance; I mean the "Principles of Geology," recently published by one whom I feel happy to be privileged to call my friend, Mr. Lyell. The great interest of this treatise seems to me to arise from its necessary tendency to force the current of scientific attention (so far as this subject is concerned) to certain points of theoretical inquiry, for the investigation of which the science has been for some time growing more and more mature, from the gradual accumulation of facts by observation and description; though, as I still think, very much still remains to be done in this humbler path of induction, before a foundation of evidence sufficiently ample to support securely the superstructure of theoretical system can be considered as fully laid. Insufficient, however, as they may be to lead to anything like final conclusions, the real nature and value of our present materials can only be fairly appreciated by so classing and arranging them as to show the exact bearing of each upon the points of theoretical inquiry. This labour has certainly been too much neglected of late years, although it is only during this period that observation has been sufficiently extended to enable us even to commence the task with any prospect of utility. Now Mr. Lyell's work is, above any others that I have seen, calculated to open this field; but it is evident that it can be cultivated only by free discussion. At first the induction must of necessity be often incomplete, and the application incorrect: mutual, open and candid, yet

yet friendly explanations of the opposing conclusions, to which the varying views of the phænomena presented to different minds give rise, can alone advance the progress of truth. It is in this view that I myself regard the occasionally strong expressions of Mr. Lyell, concerning the views of the school of geologists which he opposes; such for instance, as "They are not content with disregarding the analogy of the present course of nature when they speculate on the revolutions of past times, but they often draw conclusions concerning the former state of things directly the reverse of those to which a fair induction from facts would infallibly lead them." When I confess myself to have been led generally to adopt the opinions of the school thus condemned, by the estimate which the constitution of my own mind has forced me to form of all the phænomena which my own limited means of observation have enabled me to investigate, I trust he will view with the same indulgence my endeavours to retort, as well as I may, the charge of false analogy and incorrect induction.

If you, Mr. Editor, are therefore willing to open your pages to the prosecution of the subject, I shall be happy to avail myself of them, to endeavour to lay open my views, as to the precise arrangement and classification of the geological facts hitherto ascertained, which seem to me best calculated fairly and fully to generalize them, and to dispose them in such an order as to form the basis of a sound inductive process,—a task as yet, I think, very imperfectly performed; and an attempt to execute which must, if conducted with the least ability, point out the extent to which our present materials are defective, and suggest the inquiries requisite to supply those deficiencies. It is thus that, in the words of Leonardo da Vinci, "theory is the general, and experiments" (or in this case observations) "the soldiers." Theory will always become necessary at a certain period in the progress of every science "to generalize and consolidate" (as the writer who quotes the above axiom in a late able and interesting Life of Galileo well observes) "past observations, and thence to conjecture the course of future observation most likely to reward his assiduity." It is theory which indicates what experiments or observations may justly deserve the title of luciferous in the age to which I have alluded,—that of Kepler and Galileo. Astronomy had exactly arrived at the stage of progress to require and repay this combination of theory and observation; and the station which geology has now assumed, appears to me in most of its circumstances very similar. The favourite maxim of the Geological Society, as cited with apparent approbation by Mr. Lyell, has indeed hitherto been, "That the time

time was not yet come for a general system of geology, but that all must be content for many years to be exclusively engaged in furnishing materials for future generalizations."

Mr. Lyell has now, however, himself given a tolerably strong indication that in his opinion the present season requires a bolder line of research: and as I am inclined, for the reasons just stated, to coincide in that opinion, I do not hesitate to throw myself, by the following slender contribution, on the field he has so ably opened; although my inclination, as well as a just conviction of the limits of my own powers, have hitherto confined me almost entirely to the humbler and more cautious path of what I may call Descriptive Geology, as opposed to Theoretical Geology.

For the present I must confine myself to a very few preliminary observations, reserving for a future Number (if you shall be willing to admit it) the detailed analysis on which I propose to enter of geological phænomena, so arranged as to exhibit correctly their bearing on theoretical inquiry; convinced that no real steps can be gained otherwise than by proceeding in a course of induction thus regularly, minutely, and patiently commenced.

While I fairly admit that much new and important light has been thrown on many particular facts by Mr. Lyell's book, and that it evinces throughout both a sagacity of observation and an activity of intellect, which will entitle it to the most serious attention; yet as I remain after that attention altogether unconvinced of the correctness of his general conclusions, it were merely affecting a false humility to dissemble that I record those conclusions only to show that my own interpretation of the phænomena would lead to their direct contradictories. Now whether his interpretation or mine shall eventually prove to be correct, an open and manly discussion can alone lead to any satisfactory result.

The general object of Mr. Lyell's book seems to be, by a constant employment of the expressions "existing causes" and "the uniformity of nature" (expressions on which I shall shortly have a few observations to offer), to maintain—that all the geological phænomena with which we are acquainted may, and indeed must if we reason philosophically, be accounted for by the agency not only of natural powers still existing, but also by the agency of those powers, under exactly the same modification of circumstances in every respect in which they are actually placed, and with exactly the same degree of general energy which still subsists;—that we are authorized to predict the future occurrence of such catastrophes as the deluge of one large continent, and the elevation

 of

of another, (see p. 89,) as part of the present order of nature, though from some accident or other, which it would be rather difficult to explain, we do not happen to have seen anything of the kind for the last three thousand years;—that it is absurd to consider the ages which have elapsed since the occupation of the planet by man has given rise to historical record as a period of comparative repose;—that the earthquakes which now ravage Calabria and America are as violent as any which formerly disrupted our strata (tearing them thousands of feet asunder, evidently at a single stroke*), and which raised our continents and their mountains, and want only a little more time to reproduce the same effects;—that it is altogether unphilosophical to suppose that any causes can have tended to exhaust or diminish the power of these disturbing forces;—there being, perhaps, some canon in Mr. Lyell's logic or physics, with which mine is unprovided, to the effect, that a force which has ever acted with a given power must always continue to act with the same power.

I hope it will not be considered as an invidious remark, but merely as expressing the general impression which the book has left on my mind, that it is an expanded commentary on the celebrated Huttonian axiom, that "in the œconomy of the world no traces of a beginning or prospect of an end can be discerned." Now I would not doubt for a moment on the unfounded (as I am most willing to own) moral objections which have been urged against this axiom; but considered purely as it ought to be philosophically, I have ever regarded it, and I continue to regard it, as one of the most gratuitous and unsupported assertions ever hazarded.

At present I will only further trespass on your space to add a few words on the phrases "existing causes" and "the uniformity of nature," so often employed. Mr. Lyell has many acute and useful observations on the prejudices which have impeded just reasoning on the subject. Wishing him every success in his chivalrous encounter with these *idola specûs*, I only regret that he seems to spare with some partiality one of the tribe; I mean, mistaking words for things, and philosophical expressions for philosophical arguments. I will venture on an illustration, which may exhibit how far these phrases are really available in the argument:—Infants grow at the rate of some inches a-year; but there is a popular persuasion, evidently erroneous, according to the canons of Mr. Lyell's logic, that this rate decreases, and at length stops, and

* This illustration is, indeed, not given in the work; but I cannot suppose Mr. Lyell otherwise then well acquainted with its proofs in many geological faults of large magnitude.

that

that adults cannot grow one single inch during the rest of their lives;—a tenet evidently altogether at variance with the doctrine of "existing causes" and "the uniformity of nature" as developed in the work before us.

I remain, yours, &c.

Sully, Cardiff, Aug. 20, 1830. W. D. CONYBEARE.

XXXVI. *Proceedings of Learned Societies.*

ROYAL ACADEMY OF SCIENCES OF PARIS.

1829. August 24, *MANUSCRIPTS*:—A sealed packet by M. A. Sanson;—Note from MM. François and Caventou, On the medicinal properties of the root of a Brasilian shrub of the family of the *Rubiaceæ*;— Essay On the isochronism of the spiral springs of a chronometer without steel, and various notes, by M. Houriet;—A letter from M. Bories, who requests to be placed upon the list of candidates for the vacant place of assistant professor of pharmacy at Montpellier;—The remainder of the Treatise On the manufacture of silk goods, by the late M. Paulet, presented by his son. M. Cassini made a very favourable report respecting M. A. Richard's great work on the general study of the family of the *Rubiaceæ*.— M. Dumeril also gave an advantageous account respecting the memoir of M. Roullin relating to the effects of the ergot of maize on men and animals.— M. Mathieu reported that M. Vaucher's instrument intended for tracing parallels, offered nothing remarkable. — M. Girard gave a verbal analysis of the new history of the internal navigation of France, which M. Duterns lately published.—M. Blainville read a memoir On the gauza. —At the request of M. Amussat, the reading of his labours on the torsion of arteries was resumed.—The sealed packet which contained this note was deposited on the 20th of June.—The Section of Medicine and Surgery presented the following list of candidates for the vacant place of correspondent: MM. Meckel of Halle; Fodéré of Strasburgh; Bretonneau of Tours; Abercrombie of Edinburgh; Lallemand of Montpellier; Barbier of Amiens; and Braschet of Lyons.

August 31.—*Manuscripts*:—Royal ordonnance of the 23rd of August relative to the employment of the legacies of the late M. Monthyon;—A sealed packet by M. Cottereau;—Memoir by M. Monpensier on the quadrature of the circle.

M. Meckel had the majority of votes as corresponding member. The Academy afterwards heard Meditations on Nature by M. Geoffroy-Saint-Hilaire;—A memoir by M. Cauchy, On the applications of the calculus;—And a memoir by M. Amussat, intitled, "A new process for stopping hemorrhages in wounded arteries and veins."

September 7.—*Manuscripts*:—Letter from the Minister of the Interior, containing a report of the Prefet of the Upper Rhine, respecting an earthquake felt in that department on the 7th of last August; —A letter from the Minister of War respecting an offer which had

 been

been made to Government of a process for dyeing woollen with prussian blue;—Notice respecting a watch made of rock crystal by M. Rebillier;—Memoir On a new system of *déligation* by Dr. Mayor.

M. Devoulx of Ragusa presented a memoir On the finding of longitudes;—M. de Rossel reported that the method was neither new nor correct.

The Academy heard the remainder of M. Amussat's memoir On the means of stopping hemorrhages in arteries;—Researches of M. Chevillot on the gases in the stomach and intestines of man in disease; —and a memoir by M. Barré On the communication of motion by the concussion of elastic bodies.

September 14.—*Manuscripts:*—A letter from the Minister of the Interior, who requested a report on a memoir by M. Carpentras, respecting the direction of balloons;—A letter from M. Fay, who offered himself as a candidate for the place vacant in the School of Pharmacy at Montpellier;—A letter in which M. Dubouchet states his having found a solvent for urinary calculi which does not act upon the bladder;—A memoir On mediate percussion, by M. Piorry;—Medico-legal considerations respecting the management of the Insane, by M. Brierre de Bismont;—A description of some improved machines for the use of the blind in writing, by M. Charles Barbier; —The goldsmith and jeweller's *Vade mecum*, by M. Fessard.

The Academy heard a memoir by M. Jobert respecting the fact of the division of the soil into a great number of beds of different natures;—A memoir by M. Rigal, On some processes in lithotrity; —lastly, A dissertation On the inundation of St. Petersburgh, by M. Raucourt.

September 21.—*Manuscripts:*—Note respecting primitive roots, by M. Berthevin;—Notice by M. Payen respecting the hardness which sometimes occurs in sulphate of lime;—A memoir from Dr. Mayor of Lausanne on a method of moving invalids;—M. Geoffroy-Saint-Hilaire read a very favourable report respecting the labours of the scientific commission sent to the Morea; and M. Brongniart made a special report concerning the interesting geological researches of M. Virlet.

September 28.—*Manuscripts:*—A letter from Mr. Robert Grand to M. Geoffroy containing the figure and description of an egg which was found in a hole from which an ornithorinchus was observed to go out; it was considered as coming from this quadruped;—Theoretical considerations respecting the fossil-bone caverns of Bize, by M. Tournal;—A sealed note from M. Dutrochet;—A letter from M. Kupffer to M. Arago on an ascent of Mount Elbruz.

M. Cuvier read a very favourable report respecting the results of Dr. Bélanger's overland journey from India.

M. Leroy d'Etioles read a memoir On the retention of the urine occasioned by disease of the prostate, and on the paralysis of the bladder.

October 5.—*Manuscripts:*—A letter from M. Legrand On a case of scrofula cured by preparations of gold;—A letter from M. Foureau de Beauregard, stating that the preparations of rhatany had succeeded in

in curing the yellow fever at Vera Cruz, under the care of Dr. Chabert;—A letter from M. Aldini, who requested the appointment of a commission to examine the methods by which he proposed to protect firemen from being burnt;—A letter from M. Antomarchi on the non-communication of the lymphatic vessels and the capillary vessels;—Note from M. Payen on the limits of the temperature at which native sulphate of lime loses its water of crystallization;—M. Lisfranc read a memoir On superficial cancers which had been supposed to be deep, and observations on the cases in which persons have been preserved from the amputation of important organs.—M. Chabrier read a memoir On the means of travelling in the air.

The Section of Chemistry gave in the following list of candidates for the place of assistant professor of pharmacy at Montpellier: M. Balard; M. Regimbeau, sen.; and M. Bories.

October 12.—*Manuscripts*:—A sealed packet from M. Caillot;—Note from M. Niles respecting two young men, aged 18 years, united by the umbilicus;—A letter from M. de Humboldt to M. Arago, containing an abridged account of his important journey to Siberia, and towards the frontiers of Mongolia.—M. Cuvier read a memoir On a new kind of intestinal worm, which is named *hécatoncotyle*.—M. Poisson read a memoir On the equilibrium and movement of solid, elastic and fluid bodies.—M. Mathieu read a report respecting M. Chauvin's scale for facilitating the making of plans: the instrument is good, but must be very costly.—M. Lisfranc read a memoir On the excision of the lower part of the rectum.

M. Balard was unanimously elected to the place vacant in the School of Pharmacy at Montpellier.

October 19.—*Manuscripts*:—A sealed packet from M. Baudeloque, containing new processes relating to midwifery;—A sealed packet from M. Pelletier relating to some unfinished chemical products;—A sealed packet from M. Samuel Bauss of Vevey, containing chemical products;—A memoir On the property of projectile force in the constitution of simple bodies, by M. Ménier d'Aleth.—M. Robineau Desvoidy announced that in opening a viper (usually called the *red serpent*), he found more than 3000 young ones in various states in the uterus.—M. Geoffroy-Saint-Hilaire reported respecting twins, aged 18 years, who are attached by the belly.—M. Dupetit Thouars gave an account of the processes proposed by M. Gautheron for the instantaneous production of the figures of plants, leaves and flowers.—M. Sturm read a memoir, intitled "A new theory relating to a class of transcendental functions."

October 26.—*Manuscripts*:—A sealed packet from MM. Audouin and Milne Edwards;—Memoir respecting an hydraulic machine, by M. Sallier;—Note respecting a repeating circle by M. Lenoir, senior;—A request from M. Morlet that the Academy would give an account of a work which he had presented on the displacing and change of form in the magnetic equator.

The Academy afterwards heard a report by M. Brongniart on a memoir of M. Beaumont's, On the relative ages of the different mountain chains of Europe;—A very favourable report by M. Cuvier on the

the zoological labours executed during the journey of M. d'Urville; —And lastly, a report by M. Chevreul on a memoir by M. Robiquet On the colouring principle of lichen.—M. Becquerel read a memoir On the metallic sulphurets, iodides, bromides, &c.

November 2.—*Manuscripts:*—Observations by M. Chauvin respecting his scale of proportions;—Notice respecting an eye-water, by M. Guyon;—Memoir On the rectification of curves, by John Walsh;—Memoir by M. Babinet On the cause of the retardation of light in refringent media;—New researches on the *Crustaceæ*, by M. Milne Edwards;—A sealed packet from MM. Robiquet, Colin and Lagier, containing new observations on madder;—Memoir by M. Héricart de Thury On the project of bored wells at Lyons;—Letters from various candidates for vacant places.—M. Breschet read a memoir On the structure of the organ of hearing in some fishes. —M. Roux read a memoir containing a statement of some facts in practical surgery, in which new means were employed.

SOUTH AFRICAN INSTITUTION.

We have been favoured with two Numbers of a work, which it has given us very great pleasure to see,—The South African Quarterly Journal, published at Cape Town, and to be had also of Mr. Richardson in Cornhill. The work is intended to be auxiliary to the *South African Institution;* the object of which is "the promotion of knowledge in all that relates to the Natural History and geographic, physical, and œconomic statistics of South Africa." Of this Society His Excellency the Governor, Sir G. L. Cole, is the Patron; and the following is the list of Council and Officers elected in June last:—

President: The Honourable Lieut.-Colonel Bell, C.B.—*Vice-Presidents:* Rev. F. Fallows, F.R.S.; J. A. Joubert, Esq. LL.D.; A. Oliphant, Esq.; the Hon. J. W. Stoll.—*Treasurer:* F. S. Watermeyer, Esq.—*Secretaries:* Andrew Smith, M.D.; Rev. J. Adamson, D.D.—*Council:* The Office-bearers; and Major Mitchell; M. F. Hertzog; M. van Breda; Charles Ludwig, Esq.; R. Dyce, M.D.; Clerk Burton, Esq.; J. Murray, M.D.; Major Cloete; J. Makrill, Esq.

August 11.—Four subjects for Essays were agreed on of interest to the colony, for which medals are to be awarded.

In the author of the paper first read we are gratified to recognize Mr. J. Bowie, whose numerous discoveries have from time to time been announced in our Journal by our friend Mr. Haworth.

August 31.—Read "Remarks on the Advantages of having a Botanic Garden near Cape Town." By Mr. Bowie.

"Sketches of the Botany of the Cape District," No. 1. By Mr. Bowie.—Containing a catalogue of the indigenous plants which may be expected to flower in the month of September: with remarks on their peculiarities, uses, &c.

"Observations on the Origin and History of the Bushmen." By Dr. Smith.—In this paper the writer adduces reasons for believing that Bushmen

Bushmen existed even long before Europeans visited South Africa, and that they had possibly been coeval with the Hottentots themselves. He mentions that communities or families, of a character similar to what we understand by the term "Bushmen," inhabit all the barren wastes of Great Namaqualand, and conduct themselves towards the Hottentots and Damaras in their vicinity, exactly as those immediately in advance of our frontier do towards the colonists. It was then stated, that the majority of them are decidedly of the genuine Hottentot race; and, after some details in regard to their mental character, external physiology, and modes of living, hunting, conducting their depredations, &c. the paper concluded with "an earnest recommendation to such members as may have been in the habit of observing our savage tribes, to embody their remarks for occasions like the present," as tending to personal and general benefit.

Sept. 30.—Read, "Sketches of the Botany of South Africa," No. 2. By Mr. Bowie.—The author stated, that the number of plants indigenous to South Africa was unknown, but, to his practical knowledge, the Cape colony contained more species of phænogamous plants than have been allotted to the whole of Africa by the most complete though conjectural calculations on record. He continued by observing, that however careful the botanist might be in his researches, he would find, by visiting the same grounds in the corresponding seasons of different years, many plants which had hitherto escaped his notice altogether; and, in conclusion, furnished a list of 244 plants belonging to 99 genera, which might be expected to flower in the Cape District during the months of October and November.

"Notes on the Earthquakes which occurred at the Cape of Good Hope during the month of December 1809, &c." By Mr. von Buchenroder.—In this communication the author gave a full detail of the effects of the various shocks, more particularly at Cape Town, Jan Beesjie's Kraal, and Blaauweberg Valley; and also furnished a minute register of the barometer, thermometer, and winds, between the 4th and 27th of the month, in which the phænomena in question took place.

A paper was read, entitled "Remarks on the Phocæ or Seals met with on the coasts of South Africa, with other observations." By Mr. Jardine.

"Sketches of the Botany of South Africa," No. 3. By Mr. Bowie.—The author, after a variety of general remarks, concluded with a list of the plants that might be expected to flower in the Cape District during the months of December, January, February, and March.

"A Visit to some of the Caffre Tribes beyond the Colony." By Mr. Gill.—The hordes of Pato, Zambi, Henza, and Vosanie, came under review; and the author described at some length a variety of the manners and customs of those savages, as well as furnished a detailed account of the character of the country over which he travelled. The latter he illustrated by a plan, showing the directions and positions of the mountains, rivers, &c.

A paper

A paper was read, entitled "Experiments on Candle-wicks, and on the Effects of Chlorine upon the combustible properties of the Wax of the Candle-berry Myrtle." By Mr. Reed.

A paper "On the Exotic Plants which have been introduced into South Africa, with remarks on their Cultivation and Uses." By Mr. Bowie.

"A Description of two supposed undescribed species of Fishes." By Mr. Webster.

"A Description of the Birds inhabiting the South of Africa, &c." By Dr. Smith.

XXXVII. *Intelligence and Miscellaneous Articles.*

ALTERATION OF COLOUR IN WOOD BY OXYGEN.

M. MARCET has observed that the wood of certain trees, and especially the elm, becomes of a more or less intense red colour when exposed to the air. He has found, by a great number of experiments, that the alteration does not occur, if at the moment in which the branch is transversely cut it be placed in a perfect vacuum, or in a gas which contains no oxygen; and, on the contrary, that the colour is more intense in oxygen gas than in atmospheric air. If the wood after being cut is immersed in water, it always becomes red, even when it is immediately afterwards introduced into a vacuum, or into a gas which contains no oxygen. Elm-wood which had acquired a yellow colour, yielded it gradually to water; and this water being evaporated to dryness, the residuum, when examined, exhibited all the characters of pure tannin. As the result of his experiments, M. Marcet attributes the colouring of elm-wood to a kind of oxygenation which the tannin suffers at the moment of exposure to atmospheric air.

It is to be remarked, that in the experiments here described, the branches of the elm were always cut transversely; for if the bark be simply detached, the alteration of colour is much less distinct.—*Bib. Univ. Feb.* 1830.

DURABILITY OF STONES.

When the felspar of the granite rocks contains little alkali, or calcareous earth, it is a very permanent stone; but when in granite, porphyry, or syenite, either the felspar contains much alkaline matter, or the mica, schorl, or hornblende, much protoxide of iron; the action of water, containing oxygen and carbonic acid, on the ferruginous elements tends to produce the disintegration of the stone. The red granite, black syenite, and red porphyry of Egypt, which are seen at Rome in obelisks, columns, and sarcophagi, are amongst the most durable compound stones; but the gray granites of Corsica and Elba are extremely liable to undergo alteration: the felspar contains much alkaline matter, and the mica and schorl much protoxide of iron. A remarkable instance of the decay of granite may be seen in the hanging tower of Pisa: whilst the marble pillars in the basement remain scarcely

scarcely altered, the granite ones have lost a considerable portion of their surface, which falls off continually in scales, and exhibits everywhere stains from the formation of peroxide of iron. The kaolin or clay used in most countries for the manufacture of fine porcelain or china is generally produced from the felspar of decomposing granite, in which case the cause of decay is the dissolution and separation of the alkaline ingredients. Water is capable of dissolving in larger or smaller proportions most compound bodies; and the calcareous and alkaline elements of stones are particularly liable to this kind of operation.

When water holds in solution carbonic acid, which is always the case when it is precipitated from the atmosphere, its power of dissolving carbonate of lime is very much increased; and in the neighbourhood of great cities, where the atmosphere contains a large proportion of this principle, the solvent powers upon the marble exposed to it must be greatest. Whoever examines the marble statues in the British Museum, which have been removed from the exterior of the Parthenon, will be convinced that they have suffered from this agency; and an effect so distinct in the pure atmosphere and temperate climate of Athens, must be on a higher scale in the vicinity of other great European cities, where the consumption of fuel produces carbonic acid in large quantities.—*Jameson's Journal*, April 1830.

PREPARATION OF BROMINE AND ITS HYDRATE.

The mother-liquors containing bromine are to be evaporated to a fourth of their volume in iron pans, and then left for several days; in which time the larger part of the chloride of calcium crystallizes. The supernatant liquor, being diluted with water, is to be mixed with sulphuric acid as long as a precipitate is formed. The liquid portion being separated, and the solid residue pressed, all the fluid is to be mingled and evaporated to dryness, and then redissolved, that a certain quantity of sulphate of lime may be removed. On acting upon the solution by sulphuric acid and peroxide of manganese, and then distilling, bromine is obtained.

Hydrate of Bromine.—This compound is easily formed at a temperature of from 39° to 43° Fahrenheit, by making the vapour of bromine pass into a tube moistened with water; in about a quarter of an hour the tube is filled with solid hydrate.—*Ann. der Phys.* xiv. 485. *Roy. Inst. Journ.* April 1830.

DETECTION OF IODINE.

M. Balard's process for the detection of iodine, which consists in mixing the fluid to be examined with starch, sulphuric acid, and chlorine, is the most delicate that is known.

M. Casaseca has remarked, that when the quantity of hydriodate is very small, the blue indicating ring cannot be seen at the part where the solution of chlorine is in contact with the water containing the starch and acid; then the whole should be strongly agitated and left for a while, when the starch acquires a distinct violet colour. One part of hydriodate of potash was dissolved in two parts of distilled

 water;

water; a drop, weighing 0·0455 gramme, was put into fourteen litres of water, to which were added two grammes of starch, a little sulphuric acid, and eight drops of a solution of chlorine: in fourteen hours the starch became slightly coloured, in twenty-four hours strongly tinted of an amethystine or violet hue. Hence it appears the test thus applied is able to detect 0·0000008 of a part of iodine in solution.—*Journal de Pharmacie.*

ACTION OF ALKALIES ON ORGANIC BODIES.

Since the appearance of M. Gay-Lussac's curious paper on the above subject (see Phil. Mag. and Annals, vol. vi. p. 367), he has found that acetic acid and water were very generally produced when the caustic alkali was made to act either upon animal or vegetable substances in the manner before described.—*Roy. Inst. Journal.*

FRESH DISCOVERY OF THE CHROMATE OF IRON IN SHETLAND.

The abundance in which this ore is found as a constituent of the serpentine rock, is now adding considerably to the wealth of this remote province of Scotland. The landed proprietors continue in an active search after it, as the following extract of a letter, dated the 27th of January 1830, sufficiently shows. It is addressed to Dr. Hibbert from Thomas Gifford, Esq. of Busta, a principal landed proprietor in these islands: "I take the liberty," he writes, "of addressing a few lines to you on the subject of the chromate of iron. As you predicted, it has been found in quantity on the Ness of Hillswick and elsewhere in Northmavine."—*Brewster's Journal*, April 1830.

ON ULMIN (ULMIC ACID) AND AZULMIC ACID.

M. Polydore Boullay has published a memoir, in the *Journal de Pharmacie* for April last, on the above subject, the results contained in which have led to the following conclusions:

1. Ulmin, discovered in the products of the exudation from the elm, and since met with in turf and various other bodies, and artificially produced by M. Bracconot, also from the colouring matter of unbleached flax, occurs also in soot and vegetable matters incompletely distilled. It is also one of the usual products of the action of sulphuric and muriatic acids upon vegetable matters such as wood, starch, sugar, and alcohol.

2. Ulmin, when all its properties are considered, and especially its power of saturating bases, ought to be called *ulmic acid*. It appears to differ from the product which results from the action of air or oxygenated bodies upon extracts, tannin, gallic acid, and the gallates, on account of its colour and solubility in alcohol.

3. The composition of ulmic acid, which is equivalent to

Carbon................	56·7
Water	43·3
	100·0,

is the same as that of gallic acid, given by Berzelius, viz.

Carbon................	57·08
Water	42·92
	100·00;

but the saturating power of the ulmic acid is much weaker, the analysis of its salts showing it to be less in the proportion of 1 to 5. The small saturating power of this acid, which appears to be an excellent manure, shows how it may be plentifully conveyed to plants, by means of a very small quantity of alkaline base.

4. Notwithstanding the analogy which exists between the composition of these acids, the gallic cannot be converted into the ulmic by means of sulphuric acid. The product of their mutual action appears, on the contrary, analogous to that which results from the action of oxygenated bodies upon gallic acid and extracts.

5. The carbonaceous product which results from the spontaneous decomposition of hydrocyanic acid, does not appear to be a carburet of azote, as M. Gay-Lussac had supposed, but an hydrogenated compound, capable of combining with salifiable bases analogous to hydrocyanic acid itself.

6. The same compound appears to be reproduced, when animal matters are submitted to reactions analogous to those which convert vegetable matters into ulmic acid; such is the action of potash upon gelatin. We may, therefore, according to this analogy, which occurs also in the physical and chemical properties of these two bodies, designate it by the name of *azulmic acid*, which expresses the difference of their chemical nature.

7. Azulmic acid results not only from the spontaneous decomposition of hydrocyanic acid, but from that of hydrocyanate of ammonia; cyanogen dissolved in water, from the reaction of this gas or bases; and in a word, it occurs with all the compounds of cyanogen.

8. Pure hydrocyanic acid, by its spontaneous decomposition, appears to be converted into hydrocyanate of ammonia, and azulmic acid;—a simple result, which according to M. Boullay is easily expressed by the following formula:

$$6\,(HC^2\,AZ) = HC^2AZ + H^3\,AZ \ldots + H^2\,C^{10}\,AZ^4.$$

The result of calculation agrees perfectly with the analysis of azulmic acid, which is stated to be one volume of hydrogen, five volumes of carbon and two of azote, in dividing by 2 the formula which represents it, or by weight of

AZ^2	Azote	47·64
C^5	Carbon	50·67
H	Hydrogen	1·69
		100·00

9. The action of weak nitric acid upon cast-iron, that is, upon the very divided carbon which it contains, gives rise to an azotized matter which possesses the principal properties of azulmic acid.

10. Azulmic acid appears to combine with concentrated nitric acid, which dissolves it; and there is reason to believe that artificial

 tannins

tannins are merely compounds of this body with nitric acid, or at least that they contain a very analogous product.

NEW PROCESS FOR OBTAINING LITHIA.

M. Quesneville jun. gives the following as his method of separating lithia. "I take one part of triphane, levigated, and mix it accurately with two parts of powdered litharge: I put the mixture into a crucible, and expose it to a white heat. In about a quarter of an hour the mass is perfectly fluid; I then cool it and powder it finely: I afterwards act upon it by nitric acid, the silica separates in a very divided state; I precipitate all the nitrate of lead by sulphuric acid, and evaporate to dryness to expel all the nitric acid. I afterwards treat it with water and precipitate the alumina and other metallic oxides by ammonia, and then add carbonate of ammonia to precipitate the lime and magnesia; the solution is then filtered and evaporated to dryness. The mixture is to be strongly calcined to expel all the ammoniacal salts; this operation must not be performed in a platina crucible, as it would be acted upon; I use a porcelain one. The calcined residue is to be treated with water, and all the sulphuric acid precipitated by barytes; the filtered liquor when evaporated gives pure lithia."—*Journal de Pharmacie*, April, 1830.

ANALYSIS OF A BILIARY CALCULUS.

M. Henry was supplied with this calculus by Dr. Bally of the Hotel-Dieu at Paris; it was taken from a patient who died there; it consisted of

Animal matter, analogous to mucus, or rather to albumen	10·81
Carbonate of lime	72·70
——— magnesia, probably, traces	
Phosphate of lime	13·51
Oxide of iron, fatty matter, green colouring-matter of the bile, and loss	2·98
	100·00

It was enveloped in a glairy, yellowish viscid substance, which consisted of albumen, with traces of cholestrine, and probably also of mucus, salts, and green fatty matter of the bile.—*Ibid.*

ON POWDERING PHOSPHORUS.

M. Casaseca remarks, that the method of pulverizing phosphorus, mentioned by all chemical authors, is that of agitation for some time in water, in a well-corked bottle: but, he observes, the powder obtained by this method is very imperfect; whereas if alcohol at 36° be used instead of water, a powder of the utmost fineness is produced, which has a crystalline appearance, and on agitating the liquid in the sun, the bottle appears to be entirely filled with a light brilliant powder.—*Ibid.*

FORMIC ACID.

This acid is obtained in a state of great purity by distilling alcohol with

with sulphuric acid and peroxide of manganese; but in order to prevent the formation of sulphuric æther, it is proper to employ weak alcohol or common brandy; for if the alcohol is concentrated, there is formed not only sulphuric æther, but also formic æther, which not only diminishes the quantity of formic acid, but occasions it to give a difficultly crystallizeable and coloured salt, with lead. Acetic acid treated in the same manner does not yield formic acid; the fibrin of blood yielded some, but it was very impure.—*Ann. de Chimie*, Feb. 1830.

BI-IODATE AND TRI-IODATE OF POTASH.

M. Serullas finds that there are two acid iodates of potash,—a bi-iodate of potash formed of 2 atoms acid and 1 atom base, and a tri-iodate, consisting of 3 atoms acid and 1 atom base. The first is produced by the incomplete saturation of chloride of iodine by potash, in the form of a double crystalline compound, which being separated, dissolved, and crystallized, gives the bi-iodate.

The other results from the action of one of the following acids: sulphuric, nitric, phosphoric, muriatic and silicated fluoric upon the neutral iodate of potash; sulphuric acid is to be preferred; or it may be prepared by supersaturating potash with iodic acid.

During the incomplete saturation of chloride of iodine by potash, and consequently under the influence of excess of muriatic acid, there is formed a double compound, well crystallized and in definite proportions, of chloride of potassium and acid iodate of potash. No acid iodate or chloriodate of soda appears to exist; in order to obtain iodic acid, soda may be precipitated from the iodate by means of silicated fluoric acid, the excess of which is volatilized during the operation. This process M. Serullas prefers to Davy's by means of oxide of chlorine and iodine.—*Ibid.*

POWER OF METALLIC RODS OR WIRES TO DECOMPOSE WATER AFTER THEIR CONNECTION WITH THE GALVANIC PILE IS BROKEN.

In the experiments which I undertook in 1806-7, in company with Mr. Hisinger, we had found that rods of metal which were employed to decompose water by means of the galvanic pile continued to develope gas after their connection with the pile had ceased,—a circumstance which seemed to indicate a continuance of electrical state, though these rods showed no action upon any other portion of liquid, even of the same kind, than that in which they had been placed during their contact with the pile. This observation, which I had almost forgotten, has been lately confirmed by Pfaff, who has also added to it several others of a similar kind. We might suppose such effects to be produced by a residual polarity, both in the liquid and the metal, showing itself, as long as it continued, by a continuance of chemical action; but some of Pfaff's experiments seem to oppose this idea, for he found that the addition of ammonia to the liquid, by which all its internal polarity was destroyed, did not deprive the wires

wires of their effect. The metals which acquire this property in the highest degree are zinc and iron, next to which is gold. He attempts to explain the phænomenon by supposing that the continued passage of the electrical stream had brought the elements of the water nearer to a state of separation, so that a very slight influence was sufficient to destroy their union. It must be confessed, however, that we cannot at present advance a satisfactory explanation.—Berzelius, *Arsberättelse*, 1829, p. 33.—*Brewster's Journal.*

DETECTION OF ALLOY IN SILVER BY THE MAGNETIC NEEDLE.

Oersted has made an ingenious and novel application of the magnetic multiplier. He finds that if a good electro-magnetic multiplier with double needles be suspended by a hair or a thread of unspun silk between two pieces of wrought silver, differing only one per cent. in the quantity of copper they contain, so sensible an effect is produced upon the needle as to render this a more accurate method of proof than the common touch-stones. Small trial-plates are made of different degrees of purity, and the piece to be tried is compared with them in the following way: A thin piece of woollen cloth is dipped in muriatic acid, and laid upon the trial-plate, after which the piece to be tried is brought into contact with the acid and the wire of the multiplier. The deviation of the needle shows which contains the most alloy, and another trial-plate must be employed till the needle ceases to be affected, when both are of equal fineness. In coming to a conclusion on this point, however, several circumstances are to be taken into consideration. Wrought silver goods are generally deprived of a portion of their copper by the action of acids, so as to render the surface finer than the inner part of the metal; the proof-plates, therefore, must be prepared in the same way. Another source of error in the indications of the needle are the unequal polish and size of the two pieces of metal; the latter of these is especially difficult to overcome when the surface of the metal to be proved is not plain. When, instead of muriatic acid, a dilute solution of caustic potash is employed, and the result is unlike, it is shown that copper is not the only alloy, but that brass is present; and the potash solution renders that which contains brass so positive, that it seems considerably purer than the trial-plate. This is the case also in a very high degree when the alloyed metal contains arsenic, for example when what is called white metal has been used for an alloy. This mode of proof is exceedingly interesting in a scientific point of view, and cases may occur in which it can be employed with advantage; but the sources of error can scarcely be ever so completely done away with as to make it a practical instrument in the hands of the silversmith, as Oersted seems to expect.—Berzelius, *Arsberättelse*, 1829, p. 123.—*Brewster's Journal.*

ROYAL

ROYAL ANNUAL GRANT TO SIR J. SOUTH FOR THE PROMOTION OF ASTRONOMY.

The following is from the Times newspaper of the 5th of August.

"*Encouragement of Astronomy.—(From a Correspondent.)*

"The recent honour conferred on Sir James South originated with His late Majesty. It is well known amongst Sir James's immediate acquaintance, that he contemplated a removal to France, where he intended to transport his splendid collection of instruments; amongst which was his 20-feet achromatic telescope, which has been the object of attraction amongst the curious and the learned during the whole of the present year: and he had actually written to the French Government, who nobly and generously granted him free ingress and egress, without the payment of any duty or even examination of his packages.

"This intention being made known to His late Majesty, he was graciously pleased to express the flattering and liberal sentiments contained in the following letter, and which have been so handsomely confirmed by his royal Brother. This letter having been forwarded to the French Government by Sir James South, accompanied with his reasons for declining his intended removal to France, we are thereby enabled to obtain a copy of it: and we hail it as a symptom of the return of those halcyon days when science was honoured and protected by the Government of this country.

"'Whitehall, July 10, 1830.

"'Dear Sir,—The demise of His late Majesty, and the extraordinary press of public and parliamentary business, have compelled me to defer a communication which I should otherwise have made to you at an earlier period.

"'Shortly before the death of the late King, His Majesty signified to me his intention (if he should recover from the severe illness by which he was then afflicted) of taking the first opportunity of marking his high sense of your honourable and disinterested zeal in the cause of science, and especially of your unwearied and successful exertions to perfect and increase our knowledge of the position, distances, and relations of the heavenly bodies.

"'The King commands me to inform you, that he shall have great satisfaction in confirming the intention of his lamented Brother, and in bestowing some mark of royal favour upon one who has rendered such signal service to practical navigators.

"'His Majesty desires, therefore, that you will attend at the levee either on the 21st or 28th of this month; on which occasion His Majesty proposes to confer upon you publicly* the honour of knighthood. "'I have the honour to be, dear Sir,

"'Your obedient and faithful servant,

"'ROBERT PEEL.'

"'*James South, Esq.,*
Observatory, Kensington.'

* "St. James's Palace, July 21.—The King was this day pleased to confer the honour of knighthood upon James South, Esq. of the Observatory at Kensington."—*Gazette.*

"This

"This letter we have since understood was accompanied by another, written also by Sir Robert Peel, conveying the gratifying intelligence that His present Majesty had further been graciously pleased to place at Sir James South's disposal the sum of 300*l.* per annum, to be applied by him to the promotion of astronomy: at the same time, however, delicately stating that his acceptance of that sum would by no means lay him under any sort of obligation inconsistent with perfect independence.

"It is well known that several of His Majesty's Ministers had previously visited the Observatory at Kensington: and we understand that Sir Robert Peel, in the letter above alluded to (which does equal credit to his head and to his heart), expressed himself anxious that the country should bear some portion of the enormous expense which Sir James had incurred in pursuing his researches, not (as he says) with a view of depriving Sir James of the honour and reputation which such services insured, but 'to relieve the country from the charge of perfect indifference to subjects of a scientific nature.'"

TRIBUTE OF RESPECT TO BARON CUVIER.

The lovers of Natural History in London, anxious to do honour to a man pre-eminently distinguished in the ranks of science, gladly availed themselves of the opportunity which the visit of the Baron Cuvier to our country has afforded, by obtaining the pleasure of his company at an entertainment given at the Albion Tavern, on the 10th of August, in honour of his arrival among us. At a season of the year when so great a portion of our men of science are not resident in the metropolis, it was highly gratifying to such as were within reach to be called together on this interesting occasion. Dr. Fitton, the late President of the Geological Society, was requested by the company to fill the chair; and on his right was placed their illustrious guest. There were present, Mr. Broderip, Dr. Roget, Mr. Greenough, Rev. Dr. Goodall, General Hardwicke, Dr. Maton, M. DeCandolle, Mr. Robert Brown, Dr. Paris, Mr. Children, Mr. Charles Bell, Mr. H. Mayo, Mr. Joshua Brookes, Mr. Lowe, Mr. J. Smith, Rev. Mr. Berkeley, Dr. Horsfield, Mr. J. Bennett, Dr. Wallich, Mr. Clift, Mr. Forshall, Mr. J. F. South, Mr. T. E. Gray, Mr. Houston, Mr. Wood, Mr. Yarrell, Mr. E. T. Bennett, Mr. Thos. Bell, Mr. R. Taylor.

The day was passed most harmoniously. All were happy to render a tribute of homage to this distinguished man, who took occasion to express himself very handsomely as to the part which our country had taken in the promotion of Natural History. The company quitted the dinner-table early for the drawing-room, where the evening was passed most agreeably in general and scientific conversation.

SCIENTIFIC BOOKS.

An Introduction to Medical Botany: illustrated with Coloured Figures. By Thomas Castle, F.L.S. Member of the Royal College of Surgeons, &c.

Researches

Researches in Natural History. 2nd Edit. By John Murray, F.S.A. F.L.S. F.G.S.

A Treatise on Atmospherical Electricity, including Lightning Rods and Paragrêles. 2nd Edit. By John Murray, F.S.A. F.L.S. F.G.S.

Elements of the Economy of Nature, or the Principles of Physics, Chemistry, and Physiology; founded on the recently discovered Phænomena of Light, Electro-Magnetism, and Atomic Chemistry. By J. G. Macvicar, A.M.

Hannibal's Passage of the Alps. By a Member of the University of Cambridge.

Essay on Superstition; being an Inquiry into the Effects of Physical Influence on the Mind, in the Production of Dreams, Visions, Ghosts, and other Supernatural Appearances. By Wm. Newnham, Esq. Author of the "Principles of Physical, Intellectual, Moral, and Religious Education," &c.

An Outline of the Sciences of Heat and Electricity. By Thomas Thomson, M.D. F.R.S. Regius Professor of Chemistry in the University of Glasgow, &c.

THE MINERAL SPRINGS OF CALDAS DA RAYNHA.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

The town of Caldas in the kingdom of Portugal is situated fourteen leagues to the north of Lisbon, and about two leagues distant from the sea. It appears that several mineral springs abound in the vicinity, but the most celebrated of these are called Caldas da Raynha, a few feet elevated above the level of the Atlantic Ocean. Judging from the specimens in my possession, the geological structure of the rocky range of this district combines primitive with transition limestone. The former possesses a highly crystalline structure, and emits on collision of fragments a fœtid odour like brimstone,—a characteristic of some great marbles, as for instance the Pentelican. The latter is lucullite, belonging to the secondary series.

These mineral waters appear to be thermal, and evolve a constant vapour visible at some distance, and have from time immemorial been resorted to in inveterate rheumatism, and syphilitic and scrofulous cases, and even used internally as a tonic, with reputed success.

This mineral water is sent in considerable quantity to the Brazils, &c. in opake bottles corked and sealed.

I am indebted to the kindness of the Chevalier de Mascarenhas, Consul-general for Portugal, &c., for a specimen of the Caldas da Raynha spring, which though in quantity insufficient to determine its chemical proportionals, was yet enough to indicate the constituents; a succinct summary of which, from their interesting character, I have presumed might not be altogether foreign to your pages, nor unacceptable to many of your readers.

 This

This specimen had been carefully preserved by the Chevalier nearly two years, being hermetically sealed at the spring, in a dark glass bottle, scarcely sensible to the transmission of the rays of light. Its fœtid odour gave sufficient proof of the presence of sulphuretted hydrogen—the chief gaseous principle; there was no sediment deposited, and the water was clear and crystalline. Saline solutions of tin, lead and mercury, corroborated this evidence by their several dark-brown shades, while it was insensible to litmus, Brazil, and turmeric papers; hence the absence of free acids and alkalies was inferred as well as that of the predominance of either the acid constituent or the alkaline base of any salt. The several tests for iron were inefficient in detecting the presence of that metal, but lime was indicated by oxalate of ammonia; magnesia by phosphate of soda; a muriate by nitrate of silver, and a sulphate by nitrate of baryta.

A portion of the water, evaporated to one-fourth the former volume, was divided into equal parts: a few drops of sulphuric acid having been added to one of these, supplied instant and complete evidence of the presence of iodine, silver-leaf being tarnished, and starch assuming a deep violet tint; while a stream of chlorine passed through the other, imparted a reddish-yellow hue to it, and this being subsequently agitated with sulphuric æther separated bromine, which imparted to the supernatant stratum of æther its peculiar hyacinthine colour.

The constituents of this interesting mineral water, which may perhaps challenge competition with the most celebrated springs on the continent of Europe as to its healing virtues, appear to be sulphuretted hydrogen, sulphates of lime, &c., muriates of soda and magnesia, with hydriodate and bromide of potassa, the presence of the latter alkali being determined by the reagency of nitromuriate of platinum. The first or gaseous constituent and penultimate of these are its most valuable ingredients; and the iodine appears to be so abundant, as to promise curative success in bronchocele and other morbid glandular affections, even when applied externally as a lotion. I have the honour to remain, Gentlemen, yours, &c.

Feb. 16, 1830. J. Murray.

P.S. I may observe, that I have found the test of nitrate of silver, as proposed by Dr. John Davy, very sensible as a reagent in the detection of animal matter in water.

OCCULTATION OF ALDEBARAN BY THE MOON.

On July 15th, at $23^h\ 44^m$ apparent time, Aldebaran was observed here to disappear behind the Moon's enlightened limb, about two degrees to the left of her vertical point, and reappeared on the 16th at $31^m\ 20^s$ apparent time, a little below the centre of her western limb. Aldebaran as usual advanced upon the Moon's enlightened limb at least four seconds of time, where it was distinctly seen before it disappeared. The refraction of the atmosphere alone, it is supposed, is not sufficient to account for this phænomenon. An interesting explanation

explanation of it by M. du Sejour may be seen in Vince's Astronomy, vol. i. p. 388.

What renders this occultation of Aldebaran peculiar, is its having occurred at noon; yet with the best power of an achromatic telescope the immersion and emersion were distinctly observed.

The apparent time at Greenwich of the disappearance and reappearance of Aldebaran, as given in White's Ephemeris and Moore's Almanack, is nearly two minutes too fast; so that the time deduced from what are considered accurate astronomical computations, does not in this instance, and indeed it seldom does, agree with the time of observation.

THE PLANET MERCURY.

The planet Mercury was seen here (Gosport) with the naked eye soon after sunset in the evenings of the 12th, 15th, 16th, 18th, 23rd, 24th, and 26th instant, and on each evening his scintillation was attended with pretty strong rays of light, particularly on the 18th, when the twinkling was not much inferior to that of β Tauri, which star was about four degrees distant from him, and of the same apparent magnitude. From the twinkling appearance of this planet, it is difficult to trace him out accurately among the fixed stars of the same apparent size, when at or near his greatest western or eastern elongation, without the aid of a telescope; and then he shines like the other planets, with a steady light, and with a whitish and nearly dichotomized disc. W. B.

LIST OF NEW PATENTS.

To R.W. Sievier, Southampton Row, Russell-square, St. George's, Bloomsbury, sculptor, for certain improvements in the construction of rudders in navigating vessels.—Dated the 27th of February.—6 months allowed to enrol specification.

To S. Thompson, Great Yarmouth, Norfolk, mariner's compass-maker, for certain improvements in piano-fortes.—27th of February.—6 months.

To W. Howard, Rotherhithe, Surrey, iron manufacturer, for certain improvements in the construction of wheels for carriages.—27th of February.—6 months.

To P. C. De la Garde, Exeter, gentleman, for certain improvements in apparatus for fidding and unfidding masts, and in masting and rigging of vessels.—27th of February.—6 months.

To T. Prosser, Worcester, architect, for certain improvements in the construction of window-sashes, and in the mode of hanging the same.—6th of March.—6 months.

To T. R. Guppy, Bristol, sugar-refiner, for a new apparatus for granulating sugar.—6th of March.—6 months.

To R. Stevenson, Colridge, Stafford, potter, for improvements in machinery for making bricks, tiles, and other articles.—6th of March.—6 months.

To J. Ramsay, and A. Ramsay, Greenock, North Britain, cordage and sail-cloth manufacturers, and M. Orr, Greenock, aforesaid, sail-

maker,

maker, for an improvement in the manufacture of canvass and sail-cloth for the making of sails.—20th of March.—6 months.

To G. Scott, Water Lane, London, engineer, for certain improvements on, or additions to, windlasses and relative machinery, applicable to naval purposes.—20th of March.—6 months.

To J. A. Fulton, Lawrence Poultney Lane, Cannon-street, London, merchant, for an improvement in the preparation of pepper.—20th of March.—6 months.

To W. E. Cochrane, esquire, Regent-street, Middlesex, for an improvement, or improvements, on his patent cooking apparatus.—20th of March.—6 months.

To B. Rotch, Furnival's Inn, Middlesex, barrister-at-law, for improved guards, or protections, of horses' legs and feet, under certain circumstances.—20th of March.—12 months.

To J. Rawe, Jun. Albany-street, Regent's Park, Middlesex, being one of the people called Quakers, and J. Boase, of the same place, gentleman, for certain improvements in steam-boilers, and a mode of quickening the draft for furnaces connected with the same.—30th of March.—6 months.

To W. Aitkin, Carron Vale, Scotland, esquire, for certain improvements in the means of keeping or preserving beer, ale, and other fermented liquors.—30th of March.—6 months.

To D. T. Shears, Bankside, Southwark, Surrey, coppersmith, for certain additions to and improvements in the apparatus used in distilling, and also in the process of distilling and rectifying.—31st of March.—2 months.

To J. Collier, Newman-street, Oxford-street, St. Mary-le-bone, civil engineer, and H. Pinkus, of Thayer-street, Manchester-square, esquire, in the same parish, gentleman, for an improved method and apparatus for generating gas for illumination.—5th of April.—6 months.

To W. A. Summers, St. George's-place, St. George's in the East, Middlesex, engineer, and N. Ogle, of Millbrook, Hampshire, esquire, for certain improvements in the construction of steam-engine and other boilers, or generators, applicable to propelling vessels, locomotive carriages, and other purposes.—13th of April.—6 months.

To J. Perry, Red Lion Square, Holborn, bookseller and stationer, for an improvement or improvements in or on pens.—24th of April.—6 months.

To J. M'Innes, Aucheureoch, and of Woodburn, North Britain, esquire, for the manufacture or preparation of certain substances which he denominates the British Tapioca, and the cakes and flour to be made from the same.—24th of April.—6 months.

To S. Brown, Billiter-square, London, commander in the Royal Navy, for certain improvements in making or manufacturing bolts and chains.—24th of April.—6 months.

To J. Cochaux, Fenchurch-street, London, merchant, for an apparatus calculated to prevent or render less frequent the explosion of boilers in generating steam. Communicated by a foreigner.—24th of April.—6 months.

To

To P. Descroizilles, Fenchurch-street, London, chemist, for certain improvements in apparatus for œconomising fuel in heating water and air applicable to various purposes.—24th of April.—6 months.

To T. Cook, Blackheath Road, Kent, lieutenant in the Royal Navy, for certain improvements in the construction and fitting up of boats of various descriptions.—24th of April.—2 months.

To J. Wilks, Blue Anchor, Bermondsey, Surrey, engineer, millwright and machinist (one of the co-partners in the firm of Bryan Donkin and Co., of the same place, engineers, millwrights, and machinists), for an improvement in a part or parts of the apparatus for making paper by machinery.—28th of April.—6 months.

To T. Petherick, Penfullick, in the parish of Tywardreath, Cornwall, mine-agent, for machinery for separating copper, lead, and other ores from earthy and other substances with which they are and may be mixed, and which is more particularly intended to supersede the operation now practised or used for that purpose, commonly called Jigging.—28th of April.—6 months.

To J. Walker, Weymouth-street, Middlesex, esquire, for an improved cock for fluids.—4th of May.

To H. R. S. Devenoge, Little Stanhope-street, May Fair, Midplesex, gentleman, for certain improvements in machinery for making bricks. Communicated by a foreigner.—8th of May.—2 months.

To M. Bush, Dalnonarch, Print Field, near Bonhill by Dumbarton, North Britain, calico-printer, for certain improvements in machinery or apparatus for printing calicoes and other fabrics.—24th of May.—6 months.

To J. H. Bass, Hatton Garden, Middlesex, gentleman, for certain improvements in machinery for cutting corks and bungs.—3rd of June.—6 months.

To J. Levers, New Radford Works, near the town of Nottingham, lace machine maker, for certain improvements in machinery for making lace, commonly called bobbin net.—8th of June.—6 months.

To G. V. Palmer, parish of St. Peter, Worcester, artist, for a machine to cut and excavate earth.—8th of June.—6 months.

To W. T. Haycraft, of the Circus, Greenwich, doctor of medicine, for certain improvements in steam-engines.—11th of June. — 6 mo.

To T. Brunton, Commercial Road, Limehouse, Middlesex, merchant, and T. J. Fuller, of the same, civil engineer, for an improved mechanical power applicable to machinery of different descriptions.—19th of June.—6 months.

To R. Hicks, Conduit-street, in the parish of St. George, Hanover-square, surgeon, for an œconomical apparatus or machine to be applied in the process of baking, for the purpose of saving materials. —29th of June.—6 months.

To E. Turner, Gower-street, Middlesex, doctor of medicine, and W. Shand, of the Burn in Kincairdineshire, esquire, for a new method of purifying and whitening sugar or other saccharine matter. —29th of June.—6 months.

To M. Poole, Lincoln's Inn, gentleman, for certain improvements in the apparatus used for certain processes of extracting molasses

lasses or syrup from sugar. Communicated by a foreigner.—29th of June.—6 months.

To S. Parker, Argyle-street, Oxford-street, bronzist, for certain improvements in producing the mechanical power from chemical agents. Partly communicated by a foreigner.—29th of June.—6 mo.

To S. Parker, Argyle-street, Oxford-street, bronzist, for an improved lamp. Partly communicated by a foreigner.—29th of June.—6 months.

To R. Roberts, Manchester, civil engineer, for a certain improvement in spinning cotton or other fibrous substances.—1st of July.—6 months.

To J. H. Clive, Chell House, Staffordshire, esquire, for certain improvements in the construction of and machinery for locomotive ploughs, harrows, and other machines and carriages.—1st of July.—6 months.

To J. H. Sadler, Praed-street, Paddington, engineer, for certain improvements in looms.—1st of July.—6 months.

To M. Uzielli, Clifton-street, Finsbury-square, gentleman, for improvements in the preparation of certain metallic substances, and the application thereof to the sheathing of ships and other purposes. Communicated by a foreigner.—6th of July.—6 months.

METEOROLOGICAL OBSERVATIONS FOR JULY 1830.

Gosport:—Numerical Results for the Month.

Barom. Max. 30·35. July 27. Wind N.E.—Min. 29·44. July 3. Wind N.
Range of the mercury 0·91.
Mean barometrical pressure for the month .... 30·003
Spaces described by the rising and falling of the mercury............ 5·060
Greatest variation in 24 hours 0·460.—Number of changes 17.
Therm. Max. 84°. July 29. Wind E.—Min. 50°. Several times.
Range 34°.—Mean temp. of exter. air 63°·45. For 31 days with ☉ in ♋ 61·01
Max. var. in 24 hours 20°·00.—Mean temp. of spring-water at 8 A.M. 51·02

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the atmosphere, in the evening of the 17th... 98°
Greatest dryness of the atmosphere, in the afternoon of the 28th 50
Range of the index 48
Mean at 2 P.M. 65°·1.—Mean at 8 A.M. 72°·0.—Mean at 8 P.M. 79·6
—— of three observations each day at 8, 2, and 8 o'clock .... 72·2
Evaporation for the month 4·45 inches.
Rain in the pluviameter near the ground 1·95 inches.
Prevailing wind, S.W.

Summary of the Weather.

A clear sky, 4½; fine, with various modifications of clouds, 15; an overcast sky without rain, 6½; rain, 5.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
20	14	27	1	22	18	16

*Scale of the prevailing Winds.**

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
½	2½	1½	3½	1	12½	7	2½	31

General

General Observations. — To the 12th the weather continued showery, windy, and cold, for the height of summer, so much so as to cause serious apprehensions for the fate of the corn crops; but from that time to the end of the month it was fine and dry, and on several days hot and sultry. This providential change in the state of the atmosphere has in a short time wrought wonders, it having stopped the growth of the straw, and matured the wheat so as to make it fit for the sickle; indeed the harvest commenced here on the 30th of July, and was in active operation the first week in August.

The trite assertion that it will be "all straw and no corn," is an egregious falsehood;—a great quantity of straw there certainly is, but the wheat, barley, and oats, in this and the adjoining counties will turn out full average crops; and in most grass-land places good second crops of hay are about to be taken in. The only fear of a successful harvest is about the continuance of favourable weather, which it is sincerely hoped may hold up till all the crops are secured. The weather, however, in changeable seasons like the present one, should be studied, meteorological instruments often referred to, and every favourable opportunity be eagerly seized by those whose prosperity depends on it.

The last eight days and nights were warm; and on the 29th Fahrenheit's thermometer in the shade rose to 84°, and to fever heat in the sun's rays, —the hottest day since the 27th of June 1826. After the heat of the day, much sheet lightning emanated from the clouds from sunset till one o'clock A.M.; and also on the following evening.

The mean temperature of the external air this month is three-quarters of a degree under the mean of July for many years past.

The atmospheric and meteoric phænomena that have come within our observations this month, are one solar halo, eight meteors, two rainbows, and eleven gales of wind, namely, one from the North-east, two from the South-east, one from the South, three from the South-west, three from the West, and one from the North-west.

REMARKS.

London.—July 1. Fine in the morning: heavy rain at night. 2. Heavy rain: sultry: cloudy at night. 3. Showery. 4. Fine. 5. Cloudy and calm. 6. Fine. 7. Rain, with brisk wind. 8. Fine: very dry: clear and cold at night. 9. Cloudy: slight rain at night. 10. Fine. 11. Cloudy: rain. 12—16. Very fine. 17. Fine in the morning: rain. 18. Heavy rain. 19. Cloudy. 20. Slight rain. 21—24. Fine; at times sultry and cloudy. 25—28. Very hot. 29. Very hot: cloudy at night: lightning and rain. 30. Sultry and very hot: rain at night. 31. Cloudy in the morning: clear and fine at night.

Penzance.—July 1. Rain. 2. Fair: rain. 3. Clear. 4. Fair. 5. Rain at night. 6. Fair: rain. 7, 8. Fair. 9, 10. Clear. 11. Rain. 12. Clear. 13. Fair. 14. Clear: rain. 15. Fair. 16. Clear. 17. Rain. 18. Fair. 19. Clear. 20. Fair: misty. 21, 22. Fair: clear. 23. Fair. 24. Misty: fair. 25. Fair: clear. 26—29. Clear. 30. Fair. 31. Clear.

Boston.—July 1. Fine: rain at night. 2. Fine: rain P.M. 3, 4. Fine. 5. Cloudy. 6, 7. Fine: rain at night. 8, 9. Cloudy: rain at night. 10, 11. Cloudy. 12. Cloudy: rain early A.M. 13, 14. Fine. 15. Cloudy. 16, 17. Fine. 18. Stormy: rain early A.M. ditto forenoon. 19. Fine. 20—22. Cloudy. 23—29. Fine. 30. Fine: heavy rain, with thunder and lightning P.M. 31. Cloudy.

Meteoro-

Meteorological Observations made by Mr. BOOTH at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1830.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8¾ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.									
July 1	29·897	29·772	29·70	29·65	29·90	29·80	29·42	73	56	65	55	71	58	64	E.	S.	E.	E.	...	0·31	0·030	0·160	0·04
2	29·707	29·584	29·55	29·32	29·73	29·60	29·13	69	54	64	54	66	57	66	S.	S.	S.	S.	...	·21	·420	·440	·43
3	29·683	29·464	29·75	29·60	29·74	29·44	29·00	70	53	62	54	65	54	62	NE.	NW.	NW.	E.	0·25	·05	...	·150	...
4	29·900	29·766	29·87	29·75	29·91	29·84	29·20	75	51	64	54	68	52	63·5	SW.	NW.	SW.	NW.	...	...	...	·030	...
5	30·016	29·974	29·90	29·90	30·07	30·02	29·45	72	52	68	51	72	56	59·5	S.	S.	SW.	calm	...	...	·130	...	...
○ 6	29·974	29·805	29·90	29·88	30·02	29·88	29·40	67	57	64	56	67	57	67	S.	SW.	SW.	W.	·30	...	·050	·090	...
7	29·654	29·566	29·80	29·70	29·70	29·70	29·03	69	52	63	54	68	53	63	W.	NW.	SW.	SW.	...	·04	...	·110	·06
8	29·706	29·496	29·75	29·65	29·77	29·63	29·10	67	47	59	51	65	50	57·5	W.	NW.	W.	W.	...	...	...	·040	·10
9	29·594	29·419	29·70	29·60	29·70	29·54	28·86	65	51	62	51	66	50	54	W.	NW.	W.	NW.	·40	·01	...	·010	·05
10	29·833	29·432	29·80	29·75	29·91	29·83	29·20	66	45	64	52	66	52	58·5	W.	NW.	SW.	W.	...	...	...	·020	·03
11	29·846	29·576	29·60	29·52	29·78	29·67	29·33	68	55	67	53	67	57	55·5	E.	S.	SE.	calm	...	·09	·535	·060	...
12	29·942	29·583	29·80	29·70	29·98	29·71	29·00	68	45	64	52	67	50	63	W.	NW.	W.	W.	·25	...	...	...	·09
☾ 13	30·156	30·109	30·00	29·85	30·21	30·17	29·54	74	46	66	49	64	53	58·5	SW.	S.	W.	NW.	...	...	...	...	...
14	30·101	29·985	29·85	29·80	30·10	30·04	29·54	79	49	68	55	67	56	63	S.	S.	SE.	S.	...	...	...	...	...
15	29·984	29·970	29·90	29·85	30·04	29·99	29·28	73	53	65	56	72	56	67	SW.	SW.	SW.	W.	·50	...	·115	...	...
16	30·063	29·998	30·00	29·95	30·13	30·07	29·35	75	48	65	52	66	54	63·5	W.	NW.	W.	W.	...	...	...	...	...
17	30·049	29·883	29·85	29·85	30·10	29·96	29·40	66	60	61	54	64	59	63	S.	S.	SW.	W.	...	·22	·650	·290	...
18	29·818	29·800	29·78	29·75	29·88	29·87	29·16	66	51	64	53	62	53	62	S.	SW.	SW.	S.	·40	·47	...	·440	·11
19	30·060	29·956	30·00	29·90	30·12	30·03	29·35	72	53	64	52	67	54	63·5	S.	W.	SW.	W.	...	...	...	...	·09
● 20	30·123	30·050	30·04	30·00	30·17	30·13	29·50	68	54	62	54	67	54	61	W.	W.	SW.	W.	...	·01	...	...	...
21	30·224	30·104	30·16	30·15	30·27	30·23	29·60	78	58	68	54	72	59	64	W.	W.	NW.	NW.	·30	...	...	...	...
22	30·232	30·144	30·20	30·12	30·27	30·21	29·60	74	60	71	58	74	60	66	W.	W.	SW.	calm	...	...	...	...	...
23	30·124	30·061	30·10	30·08	30·19	30·14	29·40	75	62	68	55	73	61	67·5	W.	W.	SW.	W.	...	...	...	...	...
24	30·111	30·056	30·08	30·05	30·16	30·11	29·35	77	54	68	58	75	59	67·5	SW.	W.	W.	W.	·30	...	...	...	...
25	30·224	30·166	30·12	30·12	30·27	30·27	29·52	85	61	70	59	76	60	69	W.	W.	W.	W.	...	...	...	...	...
26	30·240	30·220	30·15	30·10	30·28	30·28	29·50	87	62	72	54	72	62	70	S.	N.	SE.	SW.	...	...	...	...	...
☽ 27	30·321	30·208	30·20	30·20	30·35	30·32	29·55	84	53	72	55	79	61	70	E.	NE.	NE.	E.	·70	...	...	...	...
28	30·323	30·235	30·15	30·12	30·32	30·26	29·66	84	60	76	60	81	66	70·5	E.	NE.	NE.	E.	·40	...	...	...	...
29	30·148	30·011	30·02	29·96	30·17	30·06	29·53	85	63	76	64	84	64	70	E.	SE.	E.	E.	·30	·03	...	·110	...
30	29·926	29·872	29·92	29·90	30·00	29·97	29·24	88	63	71	62	78	63	71·5	W.	NW.	SE.	W.	·20	·02	...	...	1·02
31	30·039	30.030	30·04	30·00	30·11	30·07	29·31	83	53	69	57	75	58	68	SW.	NW.	SW.	W.	·15	...	...	...	...
	30.323	29·419	30·20	29·32	30·35	29·44	29·33	85	45	76	49	84	50	64·1					4·45	1·45	1·930	1·950	2·02

THE
PHILOSOPHICAL MAGAZINE
AND
ANNALS OF PHILOSOPHY.

[NEW SERIES.]

OCTOBER 1830.

XXXVIII. *Further Observations on the Obliquity of the Ecliptic.* By WILLIAM GALBRAITH, *Esq. M.A.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

IN my paper published in the Phil. Mag. for July last, I endeavoured to show that the difference of the values of the obliquity of the ecliptic, derived from observations made at the summer and winter solstices, arose almost entirely from a small error in the latitude and in the tables of refraction employed in reducing the observations. The conclusion accorded well with the observations made at Greenwich, and published in Dr. Pearson's "Astronomy" till about the year 1820. Since that time, however, the subjects of the latitude and obliquity have been again discussed in the Greenwich Observations for 1826 and 1827. I have already shown that from the former the latitude should be 51° 28′ 38″·5 N., which is confirmed by Bessel. From some observations which I reduced by means of Ivory's refractions some years ago, it came out to be 51° 28′ 38″·4 N. It appears that Mr. Pond prefers the use of Bradley's refractions in reducing his observations; and consequently all his deductions must partake of the slight error attending the application of these refractions. He therefore makes the colatitude of Greenwich 38° 31′ 21″, and the latitude 51° 28′ 39″ N. being that derived by Bradley's refractions from the Observations of 1826, page 4 of the results from October, November, &c.

This result is half a second more than what I have adopted as the correct one from our best tables of refractions, and would produce a difference of 1″ between the summer and winter obliquities.

 The

The error arising from the use of Bradley's refractions would produce 1″·22 more, or 2″·22 in all. Now in the Greenwich Observations for January &c. 1827, page 13 of deductions, the mean of 15 determinations from 1812 till 1826, and reduced to 1820, gives

Summer obliquity reduced.....................	23° 27′ 46″·76
Winter obliquity.................................	23 27 44 ·39
Difference from actual results............	2 ·37
Predicted difference	2 ·22

nearly the same, the error being only −0″·15.

Now this conclusion, agreeing so nearly with our previous results, seems almost decisive of the question. There can be little doubt that the discordances between the observed obliquity at the summer and winter solstices are attributable, almost entirely, to an erroneous table of refractions for determining both the latitude and the obliquity.

To deduce the true mean obliquity from those recorded in the Greenwich Observations of 1827, it will be necessary to apply the small corrections which have been just pointed out.

Mean obliquity, summer		23° 27′ 46″·76,	winter		23° 27′ 44″·39
Errors in Bradley's refr.	+	0 ·34	——	+	1 ·56
Error in latitude.........	−	0 ·50	——	+	0 ·50
Correct mean obliquity		23 27 46 ·60	——		23 27 46 ·45

The accordance between the results is now apparent; and it is not brought about by design, but by detailing fully the principles upon which it has been obtained. Had the latitude been 51° 28′ 38″·4,—that which I deduced from Ivory's refractions, (which, so far as I have been able to judge from what comparisons I have made, are among the most accurate tables of refractions now to be found,)—the accordance would have been still closer, the summer coming out 23° 27′ 46″·50, and the winter 23° 27′ 46″·55, the mean being the same in both cases, or 23° 27′ 46″·525. This result has been derived from numerous observations on different years, reduced to 1820, and by that means may be presumed to be very correct.

It may now be proper to deduce the annual diminution in the same manner.

The mean obliquity from the observations of Bradley, Mayer, and Lacaille in 1750, was found to be 23° 28′ 18″·33

Sun's latitude with a contrary sign	+	0 ·11
Error in Bradley's refr. at summer solstice	+	0 ·34
Error in Bradley's latitude of Greenwich	−	1 ·00
Correct mean obliquity in January 1750 ...		23 28 17 ·78

This

This result is confirmed by those of Brinkley, who makes it 23° 28′ 17″·54; and of Bessel, who gives 23° 28′ 17″·65. The mean of all these three would be 23° 28′ 17″·66, almost the same as Bessel's. Since Bradley's determination was 23° 28′ 18″, agreeing exactly with Mayer's, and Lacaille's alone 1″ more, perhaps the mean of the whole, or 23° 27′ 17″·66, might be safely adopted, as the respective differences from this scarcely exceed a tenth of a second; more especially, if Bradley's alone were adopted, to which the corrections above properly apply, it would be 23° 28′ 17″·45, still deviating little from the mean which we, on the present occasion, are inclined to prefer.

Mean obliquity	in 1750	....	23° 28′ 17″·66
————	in 1820	....	23 27 46 ·525
Difference	in 70 years	....	31·135
Consequently	31″·135÷70 =	....	—0″·4448,

the annual diminution, and agrees with what I have elsewhere found from the observations of Piazzi, Bradley, and Maskelyne.

I shall next make a comparison of these results with such observations of the obliquity as appear most deserving of confidence.

Years.	Authors.	Obs. Obliquity.	Computed Obliquity.	Difference.
1750	Bradley, Mayer, and Lacaille ...	23° 28′ 17″·66		
1800	Maskelyne, Brinkley, Delambre, and Bessel......	23 27 55 ·94	23° 27′ 55″·42	—0″·52
1809	Cacciatore, Piazzi, Oriani, Arago, Pond, and Bessel	23 27 51 ·33	23 27 51 ·32	—0 ·01
1812	Woodhouse	23 27 50 ·17	23 27 50 ·08	—0 ·09
1813	Oriani, Pond, Brinkley and Arago	23 27 50 ·50	23 27 49 ·64	—0 ·86
1820	Pond, from a mean of 15 years	23 27 46 ·53	23 27 46 ·52	
Mean	Error	....	....	—0· 37

Hence it may be inferred that the mean obliquity for 1750 and for 1820 has been well determined, and that the annual diminution derived therefrom is very near the truth at the present time. I have reserved the more ancient observations for the following

Table of the Values of the Obliquity of the Ecliptic from the most remote periods for which it has been recorded; compared with that deduced from the annual variation now obtained, and applied to the obliquity of 1820.

No.	Observers.	Year.	Obs. Obliquity.	Comp. Obliquity.	Difference.
			° ′ ″	° ′ ″	′ ″
1	Eratosthenes	230 B.C.	23 51 20	23 42 58	− 8 22
2	Hipparchus	140	23 51 20	23 42 18	− 9 2
3	Ptolemy	140 A.C.	23 51 10	23 40 14	−10 56
4	Pappus	390	23 30 0	23 38 23	+ 8 23
5	Albategnius	880	23 35 40	23 34 45	− 0 55
6	Arzahel	1070	23 34 0	23 33 20	− 0 40
7	Cocheou-King	1278	23 32 12	23 31 48	− 0 24
8	Prophatius	1300	23 32 0	23 31 38	− 0 22
9	Ulug-beg	1437	23 30 27	23 30 37	+ 0 10
10	Regiomontanus	1460	23 30 0	23 30 27	+ 0 27
11	Waltherus	1490	23 29 47	23 30 13	+ 0 26
12	Copernicus	1500	23 29 24	23 30 9	+ 0 45
13	Tycho Brahe	1587	23 29 30	23 29 30	0 0
14	Cassini (father)	1656	23 29 2	23 28 59·5	− 0 2·5
15	Hevelius	1660	23 29 30	23 28 57·7	− 0 32·3
16	Cassini (son)	1672	23 28 54	23 28 52·4	− 0 2·6
17	Richer (at Cayenne)	1672	23 28 52	23 28 52·4	+ 0 0·4
18	Flamsteed (himself)	1690	23 28 56	23 28 44·3	− 0 11·7
19	Do. (accordg to Lalande)	1690	23 28 48	23 28 44·3	− 0 3·7
20	Lahire, in his Tables		23 29 0	23 28 44·3	− 0 15·7
21	Roëmer	1706	23 28 47	23 28 37·2	− 0 9·8
22	Louville	1716	23 28 31	23 28 32·8	+ 0 1·8
23	Condamine	1736	23 28 24	23 28 23·9	− 0 0·1
24	Lacaille	1750	23 28 19	23 28 17·66	− 0 1·34
25	Mayer	1750	23 28 18	23 28 17·66	− 0 0·34
26	Bradley	1750	23 28 18	23 28 17·66	− 0 0·34

From the above table it appears that the deviation of theory from observation is considerable for the observations made about two centuries ago. As the instruments employed in making observations by the early astronomers were very rude, and could not be depended upon nearer than a fourth or fifth of a degree, and as no allowance was made for refraction, because it was then unknown, such discrepancies may naturally be expected.

The first four observed obliquities deviate most considerably from the computed; but all who are aware of the state of astronomy, and of the rude manner of making observations with very imperfect instruments at those times, will not be surprised at such a circumstance. From the time of Albategnius to that of Tycho Brahe, the discrepancies are all much less, being within a minute of each other, which is as much as can be expected, since all the observations were made with instruments

struments having plane sights, till after the time of Tycho, when telescopic sights began to be introduced.

Before the year 1700, Azout and Picard applied telescopes to astronomical instruments, which have given such precision to modern observations. Accordingly it has been found that from this time observations began to possess an accuracy formerly unknown; and the differences between our theory and the observations are diminished.

By analyzing all the foregoing observations, and introducing expressions involving the squares and higher powers of the time from the assumed epoch, a formula would be obtained so as to represent all the observations with tolerable correctness. It is evident, however, that although it might represent the more early observations better, it would not represent the modern ones so well; and as the former cannot be allowed to possess great accuracy, it has been thought unnecessary to attempt to investigate such a formula, as it can be comparatively of little value when obtained. Besides, it is known from the investigations of Laplace, that the obliquity is a quantity which varies between certain limits not exceeding 3°, and as the times of maximum and minimum when the variation is nothing, are unknown, it is evident that the variation itself must be a variable quantity, and consequently it is not the same now, in all probability, that it was some thousands of years ago, or what it will be some thousands of years hence; nor have we any means of ascertaining its secular change with any degree of precision. Astronomers must therefore, in a great degree, be contented with the most accurate annual variation within a moderate number of years since the use of well-constructed instruments, and accurate methods, of reduction, have been introduced; reserving the discussion of the formula of variation for distant periods, till the lapse of ages and an accumulated mass of observations afford the means of investigating this subject with advantage.

I am, Gentlemen, yours, &c.

54, South Bridge, Edinburgh, August 10, 1830.

WILLIAM GALBRAITH.

XXXIX. *On the Occultation of Aldebaran on July* 16*th*, 1830; *and on the Accuracy of the computed Times of it given in certain Almanacks. By Mr.* THOMAS SQUIRE.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen, Epping, Sept. 14, 1830.

I BELIEVE the occultation of Aldebaran by the moon on July the 16th of the present year, was not (on account of clouds) very generally observed in this country. Nevertheless, I was

I was so fortunate as to get a good observation of the immersion at this place; and which happened at $11^h\ 56^m\ 36^s{\cdot}3$ mean solar time, or $11^h\ 50^m\ 59^s{\cdot}4$ apparent time. Now the latitude of Epping is $51°\ 41'\ 41''{\cdot}6$ N., longitude $25^s{\cdot}1$ E. of Greenwich; hence the apparent time of observation for the meridian of the latter place is $11^h\ 50^m\ 34^s{\cdot}3$, but the computed time in Moore's Almanack is stated at $11^h\ 50^m\ 26^s$, making the absolute time of immersion at Epping later than at Greenwich by about 8^s; and this is very nearly the quantity that would arise from the change in the lunar parallax. So that the computed results in White's Ephemeris and Moore's Almanack agreed (as usual) with the heavens to a great degree of exactness. I regret that clouds prevented my seeing the emersion, as I have no reason to doubt that the same happy coincidence in the computed and observed times would have been found to take place in this case as at the immersion, always relying on the general accuracy of the computations in the above and other Almanacks published by the Stationers' Company. You will, Gentlemen, judge my surprise at the concluding remarks of your Gosport correspondent W. B., where, at page 234 of your last Number, he gives what he calls the observed times of the immersion and emersion of Aldebaran in this occultation; and then unaccountably comes suddenly to the conclusion, that "the apparent time at Greenwich of the disappearance and re-appearance of Aldebaran, as given in White's Ephemeris and Moore's Almanack, is nearly two minutes too fast; so that the time deduced from what are considered accurate astronomical computations does not in this instance, and indeed it seldom does, agree with the time of observation."

Now, W. B. makes the observed time of immersion at Gosport to be July 15th at $23^h\ 44^m$; and that of emersion, July 16th at $31^m\ 20^s$! But from a little consideration relative to this phænomenon, I am confident that one, and probably both, of these observations is incorrect. However, let us see how these numbers will stand the test of the most scrupulous computation. For that purpose I will take the latitude of Gosport, as stated by Dr. William Burney, $= 50°\ 47'\ 45''$ N. and the longitude $= 4^m\ 28^s$ W. of Greenwich, from which, after a proper reduction of the latitude, and the moon's horizontal parallax for polar compression, with other necessary data, &c., I find that the immersion took place there July 15th $23^h\ 44^m\ 6^s$, and the emersion July 16th, $0^h\ 34^m\ 16^s$ apparent time, according to that meridian, instead of $23^h\ 44^m$ and $31^m\ 20^s$ respectively, as given by your correspondent. So much for the accuracy of W. B.'s observations made with the best

power

power of his achromatic telescope! By changing the above computed instants into Greenwich time, it will be seen that the variation in the moon's parallactic angle caused the absolute time of immersion at Gosport to be $1^m\ 52^s$ earlier; and that of emersion $1^m\ 3^s$ later than at Greenwich, which, from the visible position of the moon with respect to the star, &c., must eventually have been the case.

For the sake of further comparison I will here give another example or two. The emersion of Aldebaran, December 9th, 1829, P.M., was observed at Greenwich to take place at $6^h\ 46^m\ 49^s$ apparent time, and the computed result, according to Moore's Almanack, is $6^h\ 47^m\ 7^s$; difference 18^s. The immersion was not observed.

Again; on January the 6th, 1830, A.M., Aldebaran was observed at Greenwich to disappear behind the dark part of the moon at $3^h\ 37^m\ 34^s{\cdot}5$ apparent time, but the computed immersion in Moore's Almanack is $3^h\ 37^m\ 26^s$, differing from the observed time only $8\frac{1}{2}^s$. Clouds prevented the emersion being observed.

I wish I could have added more examples, but the above may suffice to show that the computations in White's Ephemeris and Moore's Almanack are much nearer the truth than W. B. would seem disposed to allow. On the 6th and 15th of October opportunities may again offer for W. B. to try his hand in this way; and in order to avoid errors in future, let me advise him not only to use his *best power*, but also his *best clock*, as correct time is allowed on all hands to be of consequence in observations of this kind!

At Epping the immersion was observed with an achromatic, and a power of 50 only; the star seemed to rest upon the moon's disc some seconds before it disappeared.

It is to be hoped that astronomers will pay attention to the occultation of Aldebaran on the 6th of next month, which will be visible in the S. and S.W. parts of England; as good observations on this important occultation, if made in places whose situations are accurately established, may afford some useful data for determining the true figure of the earth. If micrometrical measurements of the moon's diameter could be taken at the time, they would be of use.

I take this opportunity of observing that the ☌ in Æ of Aldebaran and the moon, on July 15th, as given in the Nautical Almanack, is not correct: instead of $23^h\ 21^m\ 4^s$, it should be $23^h\ 21^m\ 56^s$.

I remain, Gentlemen, yours respectfully,

THOMAS SQUIRE.

P.S.—

P.S.—The nearest appulse of the ☽'s northern border to Aldebaran on the morning of the 6th of October, at Greenwich, will be about 12″; this happens at $6^h\ 47^m\ 44^s$.

At Plymouth the ☽'s northern disc will hide the star $24^m\ 39^s$; immersion at $6^h\ 16^m\ 17^s$, and the emersion at $6^h\ 40^m\ 56^s$. The sun will rise at $5^h\ 27^m$. The above is in mean solar time, and according to the respective meridians.

T. S.

XL. *Narrative of an Excursion to the Summit of the Peak of Teneriffe on the 23rd and 24th of February* 1829. *By* Robert Edward Alison, *Esq.*

[Continued from page 200.]

AFTER resting an hour on the top of the Peak, we descended the cone in the short space of five minutes, although it had cost us 30′ in the ascent; and in about an hour we reached the Estancia. From the clearness of the atmosphere there, we found the sun very hot, and although only half-past eleven in the morning, the thermometer in the shade stood at 76°, and in the sun at 97°; but this heat was greatly owing to the local situation of the Estancia, as an hour and a half afterwards it was only 62°, at which point it generally stood on the way back, till on entering the lower region of the clouds it suddenly fell to 52°, and after to 49°, where it remained until we left our cold and wet companions behind us; and six hours after leaving the Estancia we arrived safely at the town of Orotava.

It appears to me that the difficulty of the ascent to the top of the Peak, has been greatly over-rated: from the great steepness of the cone, and the loose nature of the surface, it may present greater obstacles than the ascent of most of the Andes; but an excursion to the top of it, even at the unfavourable time of the year that I made it, cannot be compared to the danger and fatigue of an ascent of Mont Blanc. Although the extreme rarity of the air at first produced a slight feeling of sickness, a considerable quickening of the pulse, and a slight difficulty in breathing, yet these symptoms all went off after I had been a few hours on the mountain. The greatest inconvenience I felt was from a total want of appetite, and an intolerable thirst which it appeared impossible to allay, for no sooner had I taken any liquid, than it immediately returned.

It has been mentioned by some authors, that the heat of the atmosphere decreases more rapidly the further you are removed from the surface of the earth; others contend that the decrement

decrement is in arithmetical progression: but these opinions I think will be found to be incorrect.

From various observations made during expeditions to the Peak, it appears, that the decrease of temperature is more rapid in the inferior strata of the atmosphere, and slower in the superior, but at a certain high elevation (which possibly varies according to the latitude) the thermometer in the summer season is almost stationary throughout the twenty-four hours; and that even in winter, during the middle of the day at the same elevation, the temperature is nearly similar to what it is in summer, and in Teneriffe the height of this stratum of air is from nine to eleven thousand feet.

If the decrease be uniform, the mean heat of a certain elevation will be found by a thermometer placed between the lower and upper stations: but this is not the case; and from the tables which I have subjoined, it will be seen that the error is much greater when the decrease is taken in arithmetical, than in geometrical progression. In this country, the temperature of the atmosphere is said to diminish, in proportion to the height above the level of the sea, at the rate of one degree of Fahrenheit for every 270 feet of ascent. If the temperature fell in the same ratio at Teneriffe, there ought to be a difference of about 45° between the temperature of the coast and the top of the Peak, whereas the maximum difference which I experienced was only 18½°. It is true, that it is impossible to know the exact temperature of any point by a single passing observation, as the thermometer must vary every moment according to the presence of the sun, the interposition of clouds, a strong wind, or a calm: a local fog may occasion a refrigeration in that part of the atmosphere where the instrument is situated, which the rest of the air may not partake of; and any of these accidents may occur at the precise moment of observation. These can all be allowed for to a certain degree of correctness; but the immense difference between the supposed and observed decrement of heat, from the sea-coast to the top of the Peak, cannot be attributed to the effect of local causes, but must be ascribed to the incorrectness of the theory; and although it may never be submitted to accurate calculation, from the variety of disturbing causes, yet it may be brought to a near approach to truth.

It is much to be desired that some learned Society would pay attention to this problem, and resolve it by direct experiment, by establishing on the Peak a set of observations. It would be easy to find men courageous enough to undertake it: although it is covered with snow every year for the space of six

or eight months, yet the cold is considerably less than at the polar regions, where our hardy navigators were not afraid to pass the winter in the midst of the horrors of a frozen sea.

By means of a balloon, the experiment could occasionally be made, and with it they have not to fear so many of the causes of local influence, which are always difficult to determine.

In choosing a time perfectly calm, the aëronaut could raise himself almost perpendicularly, and with a velocity which he could moderate according to his wish, by having a cask of water furnished with a stop-cock, instead of bags of sand, which are generally used. The balloon might be observed from several points of its course, to be able to represent it by an equation which would enable its observers to compare the movement of the fixed thermometers with those which the aëronaut carried.

I do not know of any mountain that has been so frequently measured as the Peak of Teneriffe, and with results so different; which circumstance, I think, is caused by its peculiar local situation.

Although barometrical admeasurement has been brought to great perfection by the labours of Shuckburgh, Lindenau, Biot, Ramond, the immortal Laplace, and others, yet it is sometimes affected by certain influences, which destroy the fundamental supposition. The theory supposes the different strata of the atmosphere to be in the regular order of their density, the air to be in perfect equilibrium, and the decrease of temperature to be uniform: as the weight of the atmosphere decreases as you recede from the centre of the earth, and is proportional to the square of the distance, any particular pressure near the surface will destroy this regularity.

Barometrical observations show that a particular accumulation of atmosphere is above the Canaries; this is probably caused by a current existing in the upper regions which is opposed to a lower one. Baron Von Buch, in a memoir upon the climate of the Canaries, mentions that most travellers to the summit of the Peak have taken notice of a strong west wind blowing there so violently as hardly to allow them to stand, whilst a north-east wind prevailed below. I have frequently observed this current from the town of Orotava, when the clouds were nearly at the same elevation as the summit of the Peak, where they would accumulate in a dense body on the south-west side, whilst on the opposite point they would be broken into small parcels, which reunited after they had passed a short distance; at the same time, in the lower regions, the vapours and wind were all from the north-east. It is therefore

therefore probable that this partial pressure, which only exists in the lower regions, may be the cause of the difference of the following admeasurements of the Peak.

Barometrical admeasurements of the Peak, after the formula of Laplace.

Father Feuille in 1724	12,957 feet
M. Borda in 1776	12,646
MM. Lamanon and Monges in 1785	12,179
M. Cordier in 1803	12,284
Professor Smith in 1815	12,188
Baron Von Buch, calculated by Dr. Savinon	12,131

Geometrical admeasurements.

Father Feuille in 1724 (his base was too small)	14,159
Dr. Heberden in 1752 (several operations)	13,192
Hernandez in 1742	15,407
Borda and Pingre in 1771 (an error in the calculation)	11,337
Borda in 1776	12,188

Taken under sail.

Mauneville in 1749	12,796
Borda and Pingre in 1771	10,883
Chunuca in 1788	14,031
Johnston	12,943

[To be continued.]

XLI. *Proposed Improvements in the Construction of the Cylinder Electrical Machine and accompanying Apparatus.* By Mr. G. Dakin.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

I HAVE sent you a drawing of some improvements which I have lately made in the cylinder electrical machine for medical purposes, and which would also be found well adapted for the usual routine of electrical experiments. There is one very great drawback in the study of electricity; and that is, that many persons who are possessed of large cylindrical machines are unable to produce the effects of others of perhaps not half the size: the fact is, that all glass is not fit for this purpose, most probably owing to a slight excess of alkali in its composition. Some of the most refractory cylinders will require to be warmed, greased, and well excited with a large piece of silk or a coarse towel. Now as the common black glass, such as wine bottles are made of, appears to be well adapted for this purpose, I would recommend that cylinders, plates, jars, and rods, should be made of this sort of glass,

 and

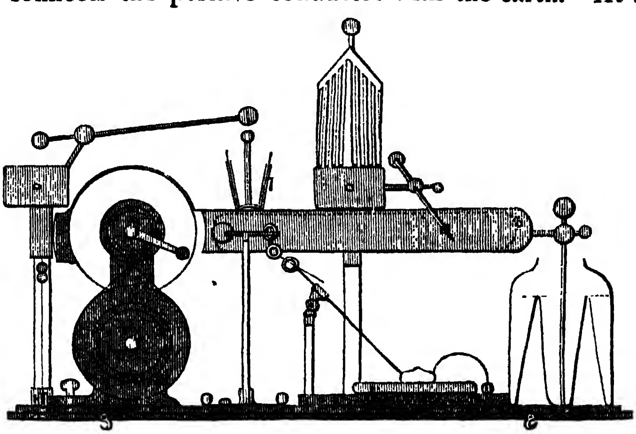
and immediately lined and covered with shell-lac, as this substance stands the very first on the list of electrics and insulators*. This practice, it appears to me, would be much better than that of insulating the cylinder, as the glass rods are so apt to get loose or broken. The cylinder represented in the figure is mounted in the usual way; but instead of terminating with the small wheel, there is a round projecting part with a bayonet catch on it, and the same on the multiplying-wheel. The crank is formed hollow like the handle of a bayonet, and fits both these; so that in case of the catgut breaking, it can be immediately fixed to the cylinder. Thus the advantages of the crank- and multiplying-wheel machine are combined; for when the amalgam is first applied, the friction is much too great for the catgut to overcome without stretching or breaking; and then it must be tightened, or the second string must be applied; and in cases of suspended animation it causes a serious interruption, even if this is at hand. The rubber is made in the usual way; but instead of a chain the communication with the earth is made with wire, and two balls, which are in contact when the prime conductor is positive, and separate or entirely removed when the negative conductor is used. With respect to the silk flap, my observation goes no further than this;—that I believe the varnished silk will keep a machine longer in moderate action in a damp room, but that unvarnished silk will adhere closer, and produces a greater effect in a dry room. The bottom of the machine should be continued the whole length of the conductor, as it contributes so much to the steadiness of the whole of the apparatus, that a person who has once been used to it would never tolerate any other. Besides, it does not take so much material to make it; for there is no necessity for a stand for the conductor, a bottom for

* In the Phil. Mag. and Annals, vol. v. p. 171, will be found an account of a globe electrical machine of uncommon power, constructed of black or green bottle-glass by the Rev. J. B. Emmett. In chemical operations, instruments and vessels of this kind of glass would be more useful even than in electricity, but the existing provisions of the Excise Laws do not permit their use: we add a paragraph on this subject from Mr. Faraday's "Chemical Manipulation," in the hope of assisting to draw attention to it, and of promoting by that means such alterations in the laws as may enable the cultivator of science to obtain apparatus formed of this useful material. Mr. Faraday observes, p. 226, "It is much to be regretted that the chemist cannot obtain glass retorts and other vessels which have to resist high temperatures, of green bottle-glass, and of all the forms and sizes he requires. Large glass retorts, for the purpose of concentrating sulphuric acid, are made of it; but much smaller ones, from two pints to an ounce in capacity, are required in the laboratory of research. Even green glass tubes are rarely to be procured, and are not permitted to be made without the special leave of the Board of Excise."—Edit.

the

the packing-case, or a bottom for the universal discharger. Another advantage is, that it is always ready for action at a moment's warning, as there is nothing to do but to lift the case off, and every thing is in its place. In Nairne's, or the most approved construction, the negative conductor is always in the way, whether it is wanted or not; and the positive conductor is not placed in the most convenient position.

As in case of charging a battery it is proper to have a small conductor, and in giving strong sparks it is necessary to have a large one, and as a negative conductor is sometimes required, the following arrangement will be found to answer for all these purposes. The best form for the conductors is oblong, of about the size of the rubber; by this construction these parts form very convenient stands of themselves: the fork and round end next the cylinder may be dispensed with, as the upper part of the positive conductor is cut with a Vandyke edge. The negative conductor is made a little smaller, so as to fit within the positive *when not wanted.* When fixed at right angles on the positive conductor, it nearly doubles the power of that; and when fixed on the rubber, it forms the negative conductor. One of its ends fits on loose, and has a shank to it: this end forms the large ball for taking strong sparks, when the small conductor is fixed vertically on the large one. The charger passes through the ball at the end of the conductor, and is fixed at any height by a screw: it also connects the negative with the positive conductor. This precludes the necessity of using the tops, balls, wires, and chains of all the jars, and they are better without them: it also connects the positive conductor with the earth. At the



end is a female screw to secure the wood and coated points, plate,

plate, balls, &c. When fixed at the end of the upper conductor, it will bring the wooden points, &c. forward into the room, and obviate the employment of the insulated handle and chain.

The discharger consists of a ball with a short rod and ring: at right angles to this is a ferule to fit the glass rod: at the other end of the rod is a ferule with a female screw. The stand is made of brass, with a large hollow edge turned up smooth. It answers three distinct purposes well, and does away with the discharging rod, which is a dangerous thing to use with a battery in the dark, and is of little use unless it be to take the residuum out of jars set also with Lane's discharging electrometer, which when fixed on the top of the jar is extremely cumbrous and liable to be broken. I have seen this latter fitted up with a micrometer screw: the absurdity of this must be evident to any practical electrician, especially if there should be the least dust in the room; in fact, the eye is quite capable of judging of any distance that is required to proportion the strength of the shock. The third purpose it answers is that of the upper plate and handle of the electrophorus: this is generally made so short, that if the operator has a moist hand it is almost useless. When used as a discharging rod, the hand is applied to the lower ferule, and the whole is moved up to any part of the conductor. When used as Lane's discharging electrometer, there is a slit and screw to fix it at any required distance: by this means it is more secure than the latter, as the twitching of the patient is apt to increase the distance of the balls without the operator being aware of it, which has often produced unpleasant consequences. When used as the upper plate of the electrophorus, the upper part is taken off, the stand also screws into the charger, and forms the upper plate for charging as tratum of air, the dancing figures, &c. By this apparatus all discharges are safely and conveniently made in the dark, whether of batteries, jars, or spiral tubes, &c.; and there will be no occasion to attach to the latter either balls or stands, if they have two hooks, one to hang in the ring and the other to touch the bottom.

The show jars of the shops are generally taken to make the Leyden jars of, the thick bottoms of which are reckoned as coated surface, though they are known to receive only weak charges. Now if this almost useless part was forced upwards so as to form an inverted jar, and the middle of this back again, as shown in the engraving, (how often this may be repeated must be left to the glass-blower,) we should have a jar that would have at least twice the power of an ordinary jar,

jar, without its weight or bulk being increased. If any means could be discovered of coating metal with glass or enamel, and *vice versâ*, a single jar might be made to have the power of an ordinary battery; and until something like this has been accomplished, the use of powerful batteries must remain in the hands of the opulent, and those of scientific bodies. I have heard of a battery of plates in the shape of a quarto volume; but however well this shape may answer on account of its portability, it is not well adapted for the purpose, as the insulating edge must go all round; but if we bend one of these plates into the shape of a cylinder, and put a bottom in, there is but one edge left to keep clean, and the other three are added to the coated surface, independently of the bottom, besides being of a much more convenient shape. A metal jar may be made and coated with sealing-wax, and the latter with tin-foil; but how far this method could be carried by inverted jars remains to be tried.

The first Henley's universal discharger that I made had two insulated wires, in the usual way; but if I had been asked the use of the second insulation, I should have been at a loss what to answer, as a jointed wire answers every purpose, and is much less in the way. The lower plate of the electrophorus is formed with a smooth round edge: it is filled with shell-lac. It forms the table of the universal discharger: when inverted and set on a drinking-glass, and a chain hooked on, it forms the lower plate for the dancing figures, &c.; without the chain, it forms the insulated stand. The resinous side will do to form Professor Lichtenberg's figures upon. The glass itself will form a jar with moveable coatings, and will do for the dancing balls, &c.

What is called the electrical pistol is a mortar or howitzer, which has to be filled with inflammable air, and fired by a separate Leyden jar: a real electrical pistol may be easily made. Take a pistol-stock and fix an ivory barrel on it with an interrupted circuit in it: then bore a hole through the stock. Take one of the smallest Leyden phials, and fit a brass tube as an outside coating; solder a wire to the coating, insert it in the hole and bend the end of the wire to form the trigger; then put a cork with a particle of fulminating silver into the barrel; charge the phial, and fire it at a mark in any part of the room; which may be done twenty times whilst the other is getting ready.

Another advantage (independent of the cheapness attending coated green glass) is that the stools and chairs would look like common ones; I have seen nervous patients thrown into a state of feverish excitement only by being placed on a glass

glass stool; they imagine that something formidable is about to happen to them, when perhaps they have only to have the electric stream drawn from the affected parts with a wooden point. When the multiplicity of apparatus required in the usual way to go through a series of experiments, and when the expensive and brittle nature of the principal materials, as well as their being often used in the dark, are considered, I flatter myself that the simplicity and advantage of the construction here recommended, will be apparent to every practical electrician; at any rate a machine so constructed will be found a very convenient "working tool."

I am, Gentlemen, yours, &c.

Dereham, Norfolk, July 15, 1830. G. Dakin.

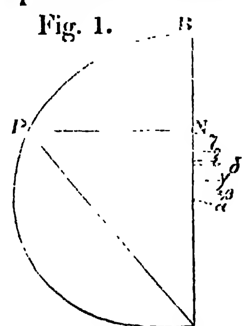
XLII. *On the Solid of greatest Attraction.* By Samuel Sharpe, *Esq.**

TO determine the form of the solid which attracts a body on the surface with the greatest force.

The attraction of each particle of the solid must be resolved into two parts, and that part rejected which is not towards the centre of gravity of the solid; and the solid is such that every point of its surface attracts the body equally towards the centre; otherwise the sum of the attractions might be increased by removing that portion which attracts least, and placing it beside that which attracts most.

Let A (fig. 1.) the body attracted, be a point on the surface of the solid.

Fig. 1.



A B ($= a$) the diameter through A and the centre of gravity.

A P ($= c$) the chord joining A and any attracting particle P.

Then letting fall PN ($= y$) perpendicular on A B, A N ($= x$) will be the corresponding abscissa.

The attraction of the particle at B $= \frac{1}{a^2}$ and of P $= \frac{1}{c^2}$, but in the direction of the diameter (this latter) $= \frac{x}{c^3}$, and therefore, $\frac{c^3}{x} = a^2$, and the equation of the curve $(x^2+y^2)^{\frac{3}{2}} = x\,a^2$ Q.—E. I.

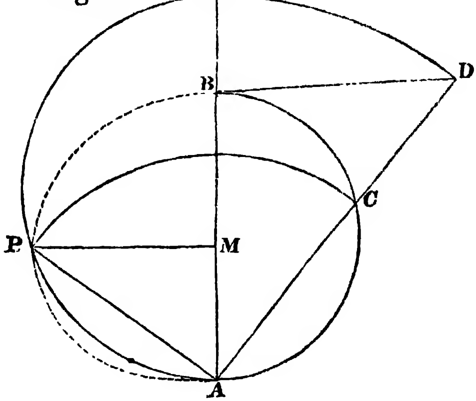
The curve may be easily drawn thus: make $z = \frac{c^2}{x}$; then

* Communicated by the Author.

$a^2 = c\,z$, and $c : a :: a : z$ and $z : c :: c : x$; from the first proportion z may be drawn, and from the second x.

On the diameter A B (fig. 2.) draw a semicircle A C B, and from A draw the chord A C produced till it meet a perpendicular from B at D; then if A C be $= c$, A D $= z$. On the diameter produced make A E = A D; on A E describe a semicircle A P E, and with the centre A and radius A C describe a circle cutting A P E in P; P will be a point in the curve required: for letting fall P M on A B,

Fig. 2.



A P or A C : A B :: A B : A D, and
A D or A E : A P :: A P : A M;

and if several other points P be thus laid down, the curve will be drawn. Q. E. D.

2. The solid is obviously made by the revolution of the area A P B on the axis A B: now to measure that area,

$y\dot{x}$ the fluxion of that area $= \overline{a^{\frac{4}{3}} - x^{\frac{4}{3}}}|^{\frac{1}{2}} x^{\frac{1}{3}}\dot{x}$ whose fluent corrected $= \dfrac{\overline{a^{\frac{4}{3}} - x^{\frac{4}{3}}}|^{\frac{3}{2}} - a^2}{2}$ which when $x = a$ becomes $\dfrac{a^2}{2}$ = the area A P B M.

3. To determine the solid contents; let $p = 3{\cdot}14159$ &c. the area of a circle whose radius is unity; then $p\,y^2\dot{x}$ is the fluxion of the solid whose fluent is $\frac{3}{5}\,p\,a^{\frac{4}{3}}\,x^{\frac{5}{3}} - \frac{1}{3}\,p\,x^3$ which when x becomes a is $= \dfrac{4\,p\,a^3}{15}$ = the whole solid contents.

4. Now since the contents of a sphere with radius b is $= \dfrac{p\,b^3}{6}$, by making $\dfrac{p\,b^3}{6} = \dfrac{4\,p\,a^3}{15}$ we obtain $a = b\dfrac{855}{1000}$ or $b = a\dfrac{1170}{1000}$, being the proportion between the axis of this solid and a sphere of equal contents.

 5. The

5. The attraction of the solid to the point A is the fluent of $2p\int \dot{x} - \frac{x\dot{x}}{c} = 2px - \frac{6pa^{-\frac{2}{3}}x^{\frac{5}{3}}}{5}$ which when x becomes a, is $= \frac{4pa}{5}$.

6. And since the attraction of a sphere to a point on its surface $= \frac{2pb}{3}$, which with equal contents $= \frac{2p}{3} \times \frac{1000\,a}{855}$ the attraction of this solid to that of a sphere is as 1026 : 1000.

7. Again, since the attraction of this solid $= \frac{4pa}{5}$, and of a sphere $\frac{2pb}{3}$ if we make their attraction equal $\frac{b}{2} = a\frac{6}{10}$.

8. Let d be the distance from A of a point on the axis, into which the whole solid might be concentrated without altering its attraction on A, then the attraction which $= \frac{4pa}{5}$ also $= \frac{4pa^3}{15} \times \frac{1}{d^2}$, and $d = a\frac{577}{1000}$, which in a sphere $=$ radius $= a\frac{585}{1000}$.

9. The distance of its centre of gravity from A is $=$

$$\frac{\text{fluent of } \frac{1}{2}py^2x\dot{x}}{\text{fluent of } \frac{1}{2}py^2\dot{x}} = \frac{\frac{3}{8}a^{\frac{4}{3}}x^{\frac{8}{3}} - \frac{1}{4}x^4}{\frac{3}{5}a^{\frac{4}{3}}x^{\frac{5}{3}} - \frac{1}{3}x^3} = (\text{when } x \text{ is} = a)\ a\frac{468}{1000}.$$

10. The ordinate is a maximum when the fluxion of $y^2 = 0 = \frac{2a^{\frac{4}{3}}\dot{x}}{3x^{\frac{1}{3}}} - 2x\dot{x}$: hence $x = a\frac{439}{1000}$ when y is a maximum.

11. Hence we obtain the following very curious points on the axis of the solid (fig. 1.), with their distances from A (a being 1000).

α. 439. The place at which its ordinate is a maximum.

β. 468. The centre of gravity.

γ. 500. The centre of the axis.

δ. 577. The point into which the whole contents might be concentrated without altering the attraction on A.

ε. 585. The centre of a sphere of equal solid contents (ε A being its radius).

ζ. 600. The centre of a sphere with equal attraction on A, a point on its circumference.

Canonbury, May 11, 1830. SAMUEL SHARPE.

XLIII. *An*

XLIII. *An Abstract of the Characters of* Ochsenheimer's *Genera of the Lepidoptera of Europe; with a List of the Species of each Genus, and Reference to one or more of their respective Icones.* *By* J. G. Children, *F.R.S. L. & E. F.L.S. &c.*

[Continued from vol. vi. p. 464.]

WE redeem our pledge, and proceed to lay before our readers an abstract of the 7th volume* of Treitschke's continuation of Ochsenheimer's *Schmetterlinge von Europa*, published towards the end of 1829. This volume contains ten genera, from *Herminia* to *Ennychia* inclusive, almost the whole of which were comprehended by Linnæus in the division *Pyralis* of his great genus *Phalæna*; "and indeed," says our author, "so closely allied are the species of that division, that only in very few instances can any doubt exist of the propriety of the place assigned to them." Linnæus only enumerates 18 species of *Pyralis* (*Syst. Nat.* edit. 12ma, Holmiæ 1767). Gmelin swells the list to 83.

Treitschke gives the following characters of the *Pyralides*.

Palpi distinct—*body* slender—*posterior feet* long—*wings* slender, the *anterior* when at rest deflexed and forming a triangle.

Larvæ small, with 14 or 16 feet—*body* thickest in the middle, generally somewhat verrucose and hairy.

Pupa long and slender.

Metamorphosis in a slight web, above ground.

Genus 107. HERMINIA, *Ochs.*, *Treitsch.*—*Latr.*

Crambus, Fabr. *Ent. Syst. Suppl.*
Polypogon, Schrank, Stephens†.

Palpi long, straight.—*Antennæ* pectinated in the male.—*Anterior wings* broad, with the posterior margin ciliated, and slightly repand.—*Body* rather stout.—*Larva* with 14 or 16 feet, slightly pilose; *hairs* short, sub-verrucose; *warts* minute ‡.

* The *sixth* vol. consists of only *two* parts.—*Tr.*

† Treitschke rejects Schrank's name *Polypogon* for this genus, in consequence of its having been previously adopted in botany: Stephens, however, has restored it.

‡ The number of feet of the larvæ of the *Pyralides* has not been sufficiently attended to: this genus might probably be divided into two sections—one consisting of those species whose larvæ have 14 feet—the other of those whose larvæ have 16.

 1. Herm.

Species.	Icon.
1. Herm. *Cribralis*, Hübn..	Hübn. Pyral. tab. 1. f. 2. (mas.)
2.—*Emortualis*, Hübn......	Hübn. Pyral. tab. 1. f. 1. (fœm.)
3.—*Derivalis*, Hübn........	Hübn. Pyral. tab. 3. f. 19. (mas.)
4.—*Grisealis*, Hübn.........	Hübn. Pyral. tab. 1. f. 4. (fœm.)
5.—*Tentaculalis*, Hübn.....	Hübn. Pyral. tab. 1. f. 6. (mas.)
6.—*Tarsicrinalis*, Hübn....	Hübn. Pyral. tab. 1. f. 5. (fœm.)
7.—*Barbalis*, Linn..........	Hübn. Pyral. tab. 19. f. 122. (mas.)
8.—*Crinalis*, Hübn..........	Hübn. Pyral. tab. 3. f. 18. (mas.)
9.—*Tarsiplumalis*, Hübn...	Hübn. Pyral. tab. 19. f. 125. (mas.)

Genus 108. HYPENA, *Ochs.*, *Treitsch.*—*Schrank.* (Stephens, Curtis.)

MADOPA, Stephens.

Palpi long, straight.—*Antennæ* setaceous; (alike in both sexes. *Head* sometimes with a conical tuft of scales projecting horizontally.—*Thorax* not large.—*Abdomen* rather slender, conical in the females.—*Wings* ample, forming a triangle when at rest; *superior* subtrigonate, acute, the anterior margin nearly straight.—*Legs* rather long. *Curtis.*)— *Larva* with 14 feet.— *Metamorphosis* takes place in a slight web between leaves or moss.

Species.	Icon.
1. Hyp. *Proboscidalis*, Linn.	Hübn. Pyral. tab. 2. f. 7. (mas.)
2.—*Crassalis*, Fabr..........	Hübn. Pyral. tab. 2. f. 12. (fœm.)
	tab. 27. f. 172. (fœm.)
	Curtis, Brit. Ent. vi. pl. 288.*
3.—*Palpalis*, Fabr.	Hübn. Pyral. tab. 2. f. 9. (mas.)
4.—*Obesalis*, Treitsch......	Hübn. Pyral. tab. 2. f. 8. (fœm.)
5.—*Antiqualis*, Hübn.......	Hübn. Pyral. tab. 23. f. 152. (fœm.)
6.—*Rostralis*, Linn..........	Hübn. Pyral. tab. 2. f. 10. (fœm.)
7.—*Obsitalis*, Hübn.	Hübn. Pyral. tab. 25. f. 164. (mas.)
	f. 165. (fœm.)
8.—*Lividalis*, Hübn.........	Hübn. Pyral. tab. 2. f. 11. (mas.)
	tab. 29. f. 186. (fœm.)
9.—*Salicalis*, Fab.†.........	Hübn. Pyral. tab. 1. f. 3. (fœm.)
10.—*Angulalis*, Hübn.	Hübn. Pyral. tab. 16. f. 107. (fœm.)

Genus 109. PYRALIS, *Ochs.*, *Treitsch.*—*Schrank.*

AGLOSSA, CLEODOBIA, Stephens.

Palpi short, straight.—*Antennæ* pectinated.—*Abdomen* of the males tufted at the posterior extremity; that of the females aculeated.

* From Curtis's figure, the first of those given by Hübner (f. 12.) is probably the most correct, especially with regard to the palpi.

† MADOPA, Stephens.

FAM.

FAM. A.—*Anterior wings* long, with a brassy hue.—*Larva* with 16 feet.

FAM. B.—*Anterior wings* shorter.—*Larva* with 14 feet. *Metamorphosis* occurs in a strong web.

FAM. A. Species.	Icon.
1. Pyr. *Cuprealis*, Hübn.*	...Hübn. Pyral. tab. 23. f. 153. (fœm.)
2.—*Pinguinalis*, Linn.*	...Hübn. Pyral. tab. 4. f. 24. (mas.)
FAM. B.	
3. Pyr. *Calvarialis*, Hübn.	...Hübn. Pyral. tab. 4. f. 23. (mas.)
4.—*Bombycalis*, Fabr.†	....Hübn. Pyral. tab. 4. f. 20. (mas.) tab. 19. f. 124. (fœm.)
5.—*Netricalis*, Hübn.	.......Hübn. Pyral. tab. 24. f. 158. (mas.)
6.—*Angustalis*, Hübn.†	....Hübn. Pyral. tab. 4. f. 21. (mas.) tab. 19. f. 123. (fœm.)
7.—*Brunnealis*, Treitsch.	...Hübn. Pyral. tab. 19. f. 126. (mas.)
8.—*Honestalis*, Treitsch.‡	
9.—*Suppandalis*, Hübn.	...Hübn. Pyral. tab. 30. f. 187. (mas.) f. 189. 190. (fœm.)
10.—*Connectalis*, Hübn.	....Hübn. Pyral. tab. 14. f. 91. (mas.)

Genus 110. SCOPULA, *Ochs.*, *Treitsch.*—*Schrank.*

SCOPULA, MARGARITIA, Stephens.
SCOPULA, Curtis.

Palpi short, conical; (distinct, curved upwards, clothed with long scales at the apex, 4-jointed.—*Labial palpi* rather long and porrected horizontally, densely covered with scales, robust, acuminated at both ends, the scales forming a pencil, and completely concealing the apical joint. *Curtis.*)—*Antennæ* setaceous, flat beneath; (sometimes as long as the wings, slender; inserted on the crown of the head. *Curtis.*)—*Abdomen* slender in both sexes; (frequently long and obtuse in the males. *Curtis.*)—*Wings* rather short, with a silky glossiness; the anterior frequently marked with light maculæ, and marginal strigæ; (various in form, the superior covering the others when at rest, and forming a triangle. *Curtis*).—*Larva* with 14 feet; (or 16. *Curtis*).—*Pupa* either inclosed in a firm earthy cocoon, or fastened between dry leaves, moss, &c.—(*Curtis.*)

FAM. A.—*Anterior wings* broad, somewhat rounded.

FAM. B.—*Anterior wings* triangular.

FAM. C.—*Anterior wings* long.

* AGLOSSA, Stephens. † CLEODOBIA, Stephens.

‡ Pyr. alis anticis flavidis, puncto medio fasciâque externâ rufis; posticis rufescentibus, basi dilutioribus.—*Ochs.*, *Treitsch.* vii. *p.* 49.

FAM. A. Species. Icon.

1.Scop.*Dentalis*, Hübn.....Hübn. Pyral. tab. 4. f. 25. (mas.)

FAM. B.

2.Scop.*Prunalis*, Wien.Verz. Hübn. Pyral. tab. 12. f. 77. (fœm.)
tab. 18. f. 118. (mas.)

3.Scop.*Sophialis*, Fabr......Hübn. Pyral. tab. 8. f. 50. (fœm.)

4.—*Pallidalis*, Hübn........Hübn. Pyral. tab. 18. f. 115. (mas.)

5.—*Frumentalis*, Linn......Hübn. Pyral. tab. 10. f. 64.

6.—*Perlucidalis*, Hübn....Hübn. Pyral. tab. 22. f. 143. (fœm.)

7.—*Nebulalis*, Hübn........Hübn. Pyral. tab. 8. f. 51. (mas.)

8.—*Pulveralis*, Hübn.*....Hübn. Pyral. tab. 17. f. 109. (fœm.)

9.—*Sticticalis*, Linn.........Hübn. Pyral. tab. 7. f. 45. (mas.)

10.—*Olivalis*, Wien.Verz.†..Hübn. Pyral. tab. 8. f. 52. (mas.)

11.—*Opacalis*, Hübn.........Hübn. Pyral. tab. 26. f. 169. (mas.)
f. 170. (fœm.)

12.—*Suffusalis*, Treitsch.‡

13.—*Alpinalis*, Fabr..........Hübn. Pyral. tab. 10. f. 63. (mas.)
tab. 27. f. 175. 176. (fœm.)

14.—*Signalis*, Treitsch. §

15.—*Nyctemeralis*, Hübn....Hübn. Pyral. tab. 22. f. 148. (fœm.)

16.—*Ænealis*, Fabr...........Hübn. Pyral. tab. 7. f. 46. (fœm.)
tab. 18. f. 120. (mas.)

17.—*Margaritalis*, Fabr.* ..Hübn. Pyral. tab. 9. f. 55. (fœm.)

18.—*Stramentalis*, Hübn.* .Hübn. Pyral. tab. 10. f. 62. (fœm.)

19.—*Longipedalis*, Dale, *MSS.*|| Curtis, Brit. Ent. vii. pl. 312.

Genus 111. BOTYS, *Ochs.*, *Treitsch.*—*Latreille.*

SCOPULA, Curtis.
HYDROCAMPA, MARGARITIA, BOTYS, DIAPHANIA, Stephens¶.

Palpi rather short, porrect.—*Antennæ* setaceous.—*Anterior wings* triangular, acuminated at the posterior angle; upper surface with a silky gloss.—*Larva* with 16 feet, generally of a yellowish or greenish colour.—*Pupa* changes in a web between dry leaves or moss.

FAM. A.—*Anterior wings* with simple, wavy, transverse lines.

* MARGARITIA, Stephens. † SCOPULA *nivealis*, Stephens.

‡ Scop. alis anticis albidis, griseo-suffusis, margine externo obscuriore; posticis dilutioribus, fasciâ externâ griseâ.—*Ochs.*, *Treitsch.* vii. p. 68.

§ Scop. alis cinereo-fuscescentibus; anticis strigâ duplici obsoletâ flavido-ferrugineâ; puncto aureo guttâque albâ.—*Ochs.*, *Treitsch.* vii. p. 70.

|| Not in Ochs., Treitsch.

¶ Besides two new genera:—one (No. 239) formed on *Botys hybridalis*, Treitsch., the other (No. 241) on *B. forficalis*, Treitsch. (*P. forficalis*, Linn.) neither named.—*Tr.*

FAM.

FAM. B.— *Anterior wings* monochromatous, or indistinctly marked with maculæ and streaks.

FAM. A. Species. Icon.

1. Bot. *Lancealis*, W. Verz.* Hübn. Pyral. tab. 18. f. 117. (fœm.)
2. —*Silacealis*, Hübn........ Hübn. Pyral. tab. 18. f. 116. (fœm.) tab. 14. f. 94. (mas.)
3. —*Sambucalis*, Hubn.*†... Hübn. Pyral. tab. 13. f. 81. (mas.)
4. —*Politalis*, Fabr.......... Hübn. Pyral. tab. 10. f. 61. (mas.) tab. 21. f. 136. (mas.) tab. 29. f. 183. (fœm.)
5. —*Rubiginalis*, Hübn..... Hübn. Pyral. tab. 12. f. 79. (fœm.)
6. —*Verbascalis*, W. Verz.*‡ Hübn. Pyral. tab. 12. f. 80. (mas.)
7. —*Comparalis*, Hübn...... Hübn. Pyral. tab. 19. f. 127. (fœm.)§
8. —*Ophialis*, Treitsch. ||
9. —*Carnealis*, Treitsch. ¶
10. —*Ochrealis*, Hübn.*‡ ... Hübn. Pyral. tab. 22. f. 146. (mas.)
11. —*Ferrugalis*, Hübn.*‡ Hübn. Pyral. tab. 9. f. 54. (fœm.) tab. 23. f. 150. (fœm.)
12. —*Fulvalis*, Hübn.......... Hübn. Pyral. tab. 22. f. 147. (fœm.)
13. —*Fuscalis*, Wien. Verz.* Hübn. Pyral. tab. 10. f. 66. (fœm.)
14. —*Cinctalis*, Treitsch.*[a] (P. *Limbalis*, Hübn.) { Hübn. Pyral. tab. 11. f. 72. (mas.) f. 73. (fœm.)
15. —*Flavalis*, Fabr. *‡ ...... Hübn. Pyral. tab. 11. f. 69.
16. —*Hyalinalis*, Hübn. *‡. Hübn. Pyral. tab. 11. f. 74. (fœm.)
17. —*Verticalis*, Linn.*‡ ..... Hübn. Pyral. tab. 9. f. 57. (mas.)
18. —*Pandalis*, Hübn........ Hübn. Pyral. tab. 9. f. 59. (mas.)
19. —*Trinalis*, Fabr........... Hübn. Pyral. tab. 11. f. 68. (mas.)
20. —*Urticalis*, Hübn.*[b] ... Hübn. Pyral. tab. 12. f. 78. (mas.)

FAM. B.

21. Bot. *Hybridalis*, Hübn.*[c] Hübn. Pyral. tab. 17. f. 114. (fœm.)
22. —*Terrealis*, Treitsch.[d]
23. —*Limbalis*, Wien. Verz. (P. *Rusticalis*, Hübn.) { Hübn. Pyral. tab. 18. f. 121. (fœm.)
24. —*Polygonalis*, Hübn.... Hübn. Pyral. tab. 10. f. 67. (mas.)

* SCOPULA, Curtis. † HYDROCAMPA, Stephens.
‡ MARGARITIA, Stephens.
§ Erroneously numbered 126 on the Plate.
|| Bot. alis albidis, atomis strigisque duabus, exteriore angulatâ, fuscis; —anticis maculis duabus fusco cinctis.—*Ochs.*, *Treitsch.* vii. p. 90.
¶ Bot. alis ex rufo albidis, strigis duabus fuscis, margine externo ferrugineo.—*Ochs.*, *Treitsch.* vii. p. 91. [a] MARGARITIA *limbalis*, Stephens.
[b] BOTYS, Stephens.
[c] NOV. GEN. No. 239.—Stephens, *Syst. Cat.* ii. p. 164.
[d] Bot. alis cinereo-fuscis, margine obscurioribus, anticis maculis obsoletis fuscis.—*Ochs.*, *Treitsch.* vii. p. 110.

25. Bot.

Species.	Icon.
25 Bot. *Diversalis*, Hübn.*†.	Hübn. Pyral. tab. 16. f. 102. (mas.)
26.—*Palustralis*, Hübn......	Hübn. Pyral. tab. 20. f. 129. (mas.) f. 131. (fœm.)‡
27.—*Unionalis*, Hübn. § ...	Hübn. Pyral. tab. 20. f. 132. (fœm.)
28.—*Palealis*, Fabr. *†......	Hübn. Pyral. tab. 11. f. 70. (fœm.)
29.—*Sulphuralis*, Hübn......	Hübn. Pyral. tab. 26. f. 166. (mas.) f. 167. (fœm.)
30.—*Turbidalis*, Treitsch. ‖	
31.—*Gilvalis*, Hübn..........	Hübn. Pyral. tab. 23. f. 154. (mas.)
32.—*Æruginalis*, Hübn......	Hübn. Pyral. tab. 20. f. 133. (fœm.)
33.—*Forficalis*, Linn. *¶	Hübn. Pyral. tab. 9. f. 58. (fœm.)
34.—*Cilialis*, Hübn.†	Hübn. Pyral. tab. 18. f. 119. (mas.)
35.—*Sericealis*, Fabr. *†	Hübn. Pyral. tab. 9. f. 56. (fœm.)

Genus 112. NYMPHULA, *Ochs.*, *Treitsch.—Schrank.*

HYDROCAMPA, Stephens.

Palpi short.—*Antennæ* setaceous.—*Abdomen* long and slender. —Upper surface of the *wings* glossy.—*Larva* with 16 feet, feed on aquatic plants.—*Pupa* changes in a cover formed of leaves.

The perfect insect haunts brooks and stagnant waters.

Species.	Icon.
1. Nymph. *Undalis*, Fabr....	Hübn. Pyral. tab. 14. f. 93. (mas.)
2.—*Interpunctalis*, Hübn.	Hübn. Pyral. tab. 19. f. 128. (mas.)
3.—*Numeralis*, Hübn.......	Hübn. Pyral. tab. 14. f. 89. (fœm.)
4.—*Punctalis*, Fabr..........	Hübn. Pyral. tab. 21. f. 140. (fœm.)
5.—*Literalis*, Hübn.[a]	Hübn. Pyral. tab. 13. f. 86. (mas.)
6.—*Nivealis*, Hübn.........	Hübn. Pyral. tab. 21. f. 141. (fœm.)
7.—*Lemnalis*, Hübn.[a]	Hübn. Pyral. tab. 13. f. 83. (mas.) f. 84. (fœm.)

* SCOPULA, Curtis. † MARGARITIA, Stephens.

‡ Erroneously numbered 130 at the bottom of the Plate.

§ Treitschke has altogether omitted Hübner's *Pyralis lucernalis*, which he says is certainly not an European species. Haworth gives it as a *Pyralis* of his section *albidales*, but with great doubt if it be "an inhabitant of Britain."—The specimen he describes, he purchased as an English one, of a London dealer; "but it is possible he might have been mistaken."—*Haworth*, Lep. Brit. p. 384. No. 27. As Stephens has not only admitted the species, but even formed a new genus, DIAPHANIA, on it, we suppose he has positive proof that it is British, and consequently that Treitschke's assertion is erroneous. By the asterisk immediately after the specific name *lucernalis*, it seems that Stephens believes the insect to have been captured *within* 25 miles of St. Paul's, though the mark is accompanied by a note of doubt.—*Tr.*

‖ Bot. alis anticis turbide stramineis; posticis dilutioribus, fasciis obsoletis fuscescentibus.—*Ochs.*, *Treitsch.* vii. p. 119.

¶ NOV. GEN. No. 241. Stephens, *Syst. Cat.* ii. p. 164.

[a] HYDROCAMPA, Stephens.

8. Nymph.

Species.	Icon.
8. Nymph. *Stratiotalis*, Hü.*	Hübn. Pyral. tab. 13. f. 87. (mas.)
9.—*Magnificalis*, Hübn....	Hübn. Pyral. tab. 16. f. 104. (fœm.)
10.—*Nymphæalis*, Treitsch.*	Hübn. Pyral. tab. 13. f. 82. (fœm.)
(*Potamogalis*, Hübn.)	
11.—*Potamogalis*, Treitsch.*	Hübn. Pyral. tab. 13. f. 85. (mas.)
(*Nymphæalis*, Hübn.)	
12.—*Fenestralis*, Hübn......	Hübn. Pyral. tab. 9. f. 60. (fœm.)

Genus 113. ASOPIA, *Ochs.*, *Treitsch.*

AGROTERA, Schrank.

PYRALIS, AGROTERA, Stephens.†

Palpi short, acuminated.—*Antennæ* setaceous.—Upper surface of the *wings* glossy; the *anterior* often with two transverse lines, inclosing a central disc of a different colour from the rest of the wing.—*Larva* little known.

FAM. A.—*Anterior wings* slightly rounded.

FAM. B.—*Anterior wings* with a party-coloured fringe, giving them an uneven, jagged appearance.

FAM. A. Species.	Icon.
1. Asop. *Farinalis*, Linn.‡...	Hübn. Pyral. tab. 15. f. 95. (fœm.)
2.—*Glaucinalis*, Linn.‡.....	Hübn. Pyral. tab. 15. f. 98. (fœm.)
3.—*Rubidalis*, Hübn.	Hübn. Pyral. tab. 15. f. 96. (fœm.)
4.—*Lucidalis*, Hübn.	Hübn. Pyral. tab. 25. f. 161. (mas.)
5.—*Corticalis*, Hübn........	Hübn. Pyral. tab. 21. f. 137. (mas.)
	tab. 24. f. 155. (fœm.)
6.—*Regalis*, Hübn..........	Hübn Pyral. tab. 16. f. 105. (mas.)
7.—*Fimbrialis*, Hübn. § ...	Hübn. Pyral. tab. 15. f. 97. (fœm.)
FAM. B.	
8. Asop. *Flammealis*, Hübn.‖	Hübn. Pyral. tab. 15. f. 99. (mas.)
9.—*Nemoralis*, Hübn.......	Hübn. Pyral. tab. 15. f. 100. (fœm.)
10.—*Incisalis*, Treitsch.......	Hübn. Tortr. tab. 1. f. 3. (fœm.)
11.—*Parialis*, Treitsch.......	Hübn. Tortr. tab. 1. f. 1. (fœm.)
	f. 2. (mas.)
12.—*Alternalis*, Treitsch.....	Hübn. Tortr. tab. 1. f. 4. (mas.)
	f. 5. (fœm.)

Genus 114. PYRAUSTA, *Ochs.*, *Treitsch.*—*Schrank.*

PYRAUSTA, Curtis.—Stephens.

Palpi short; (covered with scales, which extend far beyond the apex.

* HYDROCAMPA, Stephens.

† Also, a new genus, No. 234, which he has not yet named. Why these *nameless* genera? See *Syst. Cat.* ii. p. 160.

‡ PYRALIS, Stephens. § AGROTERA *costalis*, Stephens.

‖ NOV. GEN. No. 234. Stephens.

apex.—*Labial palpi* porrected like a beak, longer than the head, robust, covered with scales, which extend far beyond the apex. *Curtis.*)—*Antennæ* setaceous; (nearly capillary, alike in both sexes, inserted between the eyes on the crown of the head.—*Head* rather small, covered with long scales close to the forehead. *Curtis.*)—*Wings*, anterior somewhat rounded; a common band, or line of spots, frequently passing through the middle of the upper surface of each wing;—(superior covering the inferior when at rest, slightly deflexed, and forming a triangle. *Curtis.*)—*Larva* fusiform, hairy, with 16 feet.—*Metamorphosis* occurs in a papyraceous web.

Species.	Icon.
1. Pyr. *Sanguinalis*, Hübn.	Hübn. Pyral. tab. 6. f. 33. (mas.) tab. 28. f. 178. (mas.)
2.—*Castalis*, Treitsch.*	
3.—*Purpuralis*, Linn.†	Hübn. Pyral. tab. 6. f. 35. (mas.) f. 34. (mas.)
4.—*Punicealis*, Wien. Ver.†	Hübn. Pyral. tab. 6. f. 36. (fœm.)
5.—*Porphyralis*, Fabr.†	Hübn. Pyral. tab. 6. f. 37. (fœm.)
6.—*Ostrinalis*, Hübn.†	Hübn. Pyral. tab. 17. f. 113. (fœm.)
7.—*Cespitalis*, Hübn.†	Hübn. Pyral. tab. 6. f. 39. (fœm.) tab. 7. f. 40. (mas.)‡.
8.—*Rubricalis*, Hübn	Hübn. Pyral. tab. 16. f. 106. (fœm.)
9.—*Normalis*, Hübn	Hübn. Pyral. tab. 7. f. 41. (mas.) tab. 17. f. 110. (fœm.)
10.—*Stygialis*, Treitsch.§	
11.—*Scutalis*, Hübn	Hübn. Pyral. tab. 24. f. 156. (mas.)
12.—*Floralis*, Hübn	Hübn. Pyral. tab. 22. f. 142. (fœm.)

Genus 115. HERCYNA, *Ochs.*, *Treitsch.*

NOLA, Stephens.

These insects are small, generally dark-coloured, and in appearance allied to the *Noctuæ*, to which family they are referred in the Vienna Catalogue, as well as in other works.—The *antennæ* are crenate, or pectinated; the *palpi* short, and the surface of the *anterior wings* smooth and glossy, or uneven and dull.—*Larva* fusiform, hairy, with 14 feet.—*Metamorphosis* occurs in a closed case.

* Pyr. alis anticis pallidè flavis, fasciâ mediâ, strigâque externâ roseâ; posticis flavo-cinerascentibus.—*Ochs.*, *Treitsch.* vii. p. 165.

† PYRAUSTA, Curtis, Stephens.

‡ The second figure, quoted from Hübner, is his *P. sordidalis*, which both Curtis and Stephens consider as a separate species.—*Tr.*

§ Pyr. alis fusco-nigricantibus, anticis nitentibus, obsoletè flavo variegatis. —*Ochs.*, *Treitsch.* vii. p. 176.

FAM.

FAM. A.—*Antennæ* crenate; *anterior wings* smooth.
FAM. B.—*Antennæ* pectinated; *anterior wings* rough.

FAM. A. Species. Icon.

1. Herc. *Manualis,* Freyer..Frey. Beytrage, iv. heft, tab. 19. f. 2.
2.—*Holosericalis,* Hübn.... Hübn. Pyral. tab. 17. f. 112. (mas.)
3.—*Rupicolalis,* Hübn.. ... Hübn. Pyral. tab. 21. f. 139. (mas.)
4.—*Alpestralis,* Hübn....... Hübn. Pyral. tab. 21. f. 135. (mas.)
5.—*Dubitalis,* Hübn........ Hübn. Pyral. tab. 8. f. 49. (mas.)
6.—*Ambigualis,* Treitsch.*
7.—*Mendaculalis,* Treitsch.†
8.—*Ramalis,* Hübn. .... Hübn. Pyral. tab. 14. f. 92. (mas.)

FAM. B.

9.—*Strigulalis,* Hübn.‡ .... Hübn. Pyral. tab. 3. f. 16. (mas.)
10.—*Palliolalis,* Hübn.‡..... Hübn. Pyral. tab. 3. f. 13. (mas.) tab. 23. f. 149. (fœm.)
11.—*Togatulalis,* Hübn. .... Hübn. Pyral. tab. 20. f. 130. (fœm.)§
12.—*Albulalis,* Hübn......... Hübn. Pyral. tab. 3. f. 14. (mas.)
13.—*Cristulalis,* Hübn....... Hübn. Pyral. tab. 3. f. 17. (mas.)
14.—*Centonalis,* Hübn....... Hübn. Pyral. tab. 3. f. 15. (mas.)

Genus 116. ENNYCHIA, *Ochs., Treitsch.*
PYRAUSTA, Curtis. ENNYCHIA, Stephens.

These are small dark-coloured moths, somewhat resembling *Noctuæ,* whose wings have a silky lustre, both on the upper and under surfaces.—The *palpi* are short, and the *antennæ* setaceous.

FAM. A.—The upper surface of the *wings* traversed by a light-coloured transverse band common to both.
FAM. B.—The *wings* with spots, or bands.

FAM. A. Species. Icon.

1. Ennych. *Albofascialis,* Treit.‖
2.—*Fascialis,* Hübn.¶...... Hübn. Pyral. tab. 5. f. 31. (mas.)
3.—*Cingulalis,* Hübn.[a] ..... Hübn. Pyral. tab. 5. f. 50. (mas.) Curtis, Brit. Ent. iii. pl. 128.
4.—*Anguinalis,* Hübn.[a] .... Hübn. Pyral. tab. 5. f. 32. (fœm.)
5.—*Sepulcralis,* Treitsch.[b]

* Herc. alis anticis ex fusco-cinerascentibus, maculis fuscis, lineâ albâ; posticis albidis.—*Ochs., Treitsch.* vii. p. 184.

† Herc. alis anticis ex violaceo-albidis, maculis quatuor in margine antico, signo medio lineisque duabus fuscis; posticis basi albidis, margine fusco ferrugineoque adspersis.—*Ochs., Treitsch.* vii. p. 185.

‡ NOLA, Stephens.

§ Erroneously numbered 131, at the bottom of the Plate.

‖ Ennych. alis fuscis, atomis albo-virescentibus, fasciâ, fimbriisque albis.—*Ochs., Treitsch.* vii. p. 196.

¶ An Pyr. *fascialis,* Haw.?

[a] PYRAUSTA, Curtis.—ENNYCHIA, Stephens.

[b] Ennych. alis ex fusco-atris, fasciâ mediâ serieque punctorum niveis.—*Ochs., Treitsch.* vii. p. 199.

FAM. B. Species.	Icon.
6. Ennych. *Luctualis,* Hübn.	Hübn. Pyral. tab. 14. f. 88. (mas.)
7.—*Octomaculalis,* Treit.*...	Hübn. Pyral. tab. 12. f. 75. (fœm.)
8.—*Pollinalis,* Hübn.......	Hübn. Pyral. tab. 5. f. 29. (mas.)
9.—*Quadripunctalis,* Fabr..	Hübn. Pyral. tab. 12. f. 76. (fœm.)
10.—*Nigralis,* Fabr.	Hübn. Pyral. tab. 5. f. 26. (mas.)
11.—*Atralis,* Hübn.†.........	Hübn. Pyral. tab. 5. f. 27. (mas.)

End of Volume 7.

[To be continued, as soon as the next volume shall have been received.]

* Ennychia, Stephens.

† Nec *Py. atralis,* Haw. et Curtis; istæc enim species Enn. *octomaculali* Treitsch. (*Guttali,* scilicet *Hübneri*) convenit.

XLIV. *Additions to the Theory of Eclipses, and the Methods of calculating their Results**.

[1.] FOR calculating the results of eclipses two methods, founded on different principles, have been hitherto employed. The first follows up the phænomenon in the manner in which it presents itself to the observer. It requires, therefore, the calculation of the apparent distances of both the eclipsing and the eclipsed bodies from the pole, as also of their apparent semidiameters, in order to find, by means of the former together with the distance of the centres resulting from the apparent diameters, the angle at the pole; it requires besides the calculation of the change of this angle produced by parallax, in order to deduce, by applying it to the angle at the pole, the corresponding angle which would be observed in the centre of the earth; this method finally gives from the magnitude of this angle and the rate of its change as deduced from the tables of both objects, the time elapsing between the moment of the observation and that of the conjunction of both bodies, by which the latter expressed in time of the place of observation is derived. This method is very old; Kepler has already explained it; in modern times it has been developed by Lalande†, and more completely and accurately by Bohnenberger‡. Both adopt for the pole that of the ecliptic; Gerstner§, however, refers all the calculations which the method requires to the pole of the equator. Carlini|| has lastly improved this method by calculating the parallax, in the case

* From Schumacher's *Astron. Nachrichten,* No. 151.

† *Astronomie,* § 1977.

‡ *Geogr. Ortsbestimmung* (Determination of the geographical Position of Places), p. 323. § *Astr. Jahrb.* 1798, p. 128.

|| Zach's *Corresp.* vol. xviii. p. 528.

case of the eclipsed body being a fixed star, not for the centre of the eclipsing body but for the point of the limb where the occultation or emersion takes place, that is to say, for a point whose apparent place is the same with that of the star, in consequence of which, instead of the apparent diameter the true one is introduced into the calculation, and the calculation of the former as well as of the true longitude at the time of the observation is avoided. This method has called forth the various attempts to find more convenient formulæ for the effects of parallax: I do not deem it necessary to give an account of the numerous essays written with a view to this purpose.*

The second method, on the contrary, does not take notice of the single parts of the phænomenon; but consists in making the apparent distance of the celestial bodies, expressed by their geocentric places and those of the place of observation, equal to the sum or the difference of the apparent diameters of the bodies (according as the external or internal contact has been observed), thus producing an equation whose unknown quantity is the time of the conjunction. We are indebted for this method to Lagrange*, in whose memoir on this subject the unknown quantity of the equation is the moment in which both bodies have the same longitude. This method has been less frequently applied than the other; but its influence has extended far beyond the theory of eclipses, and has given a new form to the problems of astronomy in general. Clausen (in an excellent paper printed in the *Astr. Nachr.* No. 40) has chosen for the unknown quantity the time of the shortest geometrical distance.

[2.] The object of all calculations of observed phænomena of this kind is either to find the corrections required by the elements of the calculation taken from the astronomical tables, or the difference of the meridians of the places of observation.

With regard to the second object, the first method has not yet been completely developed, as the geocentric distances of the celestial bodies from the pole, and their horizontal parallaxes at the time of observation at a place whose difference of meridian is unknown, are assumed to be known. The error arising from this circumstance is of practical importance only as far as it affects the latitude (or declination) of the moon; but it may be easily corrected. If we denote the time of conjunction resulting from the calculation by τ, and the change which will be produced in it by a correction x of the difference of meridians (d) assumed in the calculation, by $a\,x$, the true time of conjunction will be $\tau + a\,x$, the corresponding time for

* Berlin Ephemeris for the Year 1782, p. 16.

the

the former meridian $= \tau - d - x + a\,x$. This value for the time compared to another (τ') resulting from observations at places whose meridians are known, will give

$$x = \frac{\tau - d - \tau'}{1 - a}, \text{ or the true difference of meridian}$$

$$d + x = \tau - \tau' + \frac{a}{1 - a}\,(\tau - d - \tau').$$

If, as usual, the effect of a change $\Delta\,\delta$ of the latitude (or declination) on the time of conjunction has been calculated and found equal to $m\,\Delta\,\delta$, and if the change of δ in a second of time be denoted by β, we have $a = m\,\beta$. It is clear, therefore, that in the usual manner of calculating in respect to the ecliptic, where β may amount to $0''{\cdot}055$, it may not unfrequently happen that the time hitherto neglected may amount to the tenth part of the error of the assumed difference of meridians. For nearly central occultations this neglect is of no consequence; but in cases of occultations of short duration the whole result may be rendered illusive by it.

The second method, on the contary, does not suppose the difference of meridians to be known, but determines it by the solution of an equation which is obtained by successive approximations, the first of which even may be made independent of an approximate knowledge of the difference of the meridians. I shall demonstrate that this advantage is not the only one which the second method possesses over the first, but that it likewise leads to the end proposed by a more easy calculation. This is obtained by introducing as an unknown quantity, besides the corrections of the elements of the calculation, the difference of the meridians of the places of observation itself, instead of taking as such the time of conjunction. Indeed the time of conjunction may be considered as an auxiliary quantity, the calculation of which is only interesting as far as it affords a means of determining the errors of the tables. If these errors are determined directly, without using the time of conjunction, the knowledge of the latter becomes entirely superfluous: if it be still required, it may be found without difficulty after the errors of the tables have been ascertained.

[3.] In the first place, I shall generally and completely develop the equations for the external and internal contacts of two spherical bodies. The symbols which I shall employ are as follows:

$\alpha,\ \delta,\ \varrho$ The right ascension, declination, horizontal radius of the nearer body.

A, D, R The same for the more distant body.

$\alpha',\ \delta',\ \varrho'$ / A′, D′, R′ — The same quantities as they appear at the place of observation.

$\mu,\ \varphi'$

μ, ϕ' The right ascension and declination of the corrected zenith.

π, π' The equatorial parallaxes of the two bodies.

Δ, Δ', r The distances of both bodies, and of the centre of the earth, from the place of observation.

I have here adopted the relations to the equator; those to the ecliptic might as well have been taken, and, more generally, any point of the sphere may be chosen, and the great circle of which that point is the pole, may be substituted for the equator, and any point of that great circle may be substituted for the point from which the right ascensions are counted. The calculation being the same for all assumptions, it is unnecessary separately to develop it for each case.

We have the following well-known equations:

$$(1)\ldots\left\{\begin{array}{lll} \Delta \cos \delta' \sin \alpha' &= \cos\delta \sin\alpha &- r\cos\phi'.\sin\pi.\sin\mu \\ \Delta \cos \delta' \cos \alpha' &= \cos\delta \cos\alpha &- r\cos\phi'.\sin\pi.\cos\mu \\ \Delta \sin \delta' &= \sin\delta &- r\sin\phi'.\sin\pi \\ \Delta' \cos D' \sin A' &= \cos D \sin A &- r\cos\phi'.\sin\pi'.\sin\mu \\ \Delta' \cos D' \cos A' &= \cos D \cos A &- r\cos\phi'.\sin\pi'.\cos\mu \\ \Delta' \sin D' &= \sin D &- r\sin\phi'.\sin\pi' \end{array}\right.$$

If we denote the first three of these quantities by a, b, c, and the latter three by a', b', c', we have directly

$$\Delta\Delta'[\cos\delta'\cos D'\cos(\alpha'-A')+\sin\delta'\sin D'] = aa'+bb'+cc',$$

and as the expression multiplied by $\Delta\Delta'$ is the cosine of the apparent distance of the centres (Σ), we have

$$\Delta\Delta'\cos\Sigma = aa' + bb' + cc'.$$

For the commencement and the end of the occultation we have $\Sigma = \varrho' \pm R'$, where the upper sign refers to an external, the lower one to an internal contact of the limbs. We have, therefore, likewise,

$$\Delta\Delta'\cos\Sigma = \Delta\Delta'\cos\varrho'\cos R' \mp \Delta\Delta'\sin\varrho'\sin R';$$

whence in conjunction with the following equations

$$\Delta\sin\varrho' = \sin\varrho,\quad \Delta\cos\varrho' = \sqrt{(a^2+b^2+c^2-\sin\varrho^2)}$$
$$\Delta'\sin R' = \sin R,\quad \Delta'\cos R' = \sqrt{(a'^2+b'^2+c'^2-\sin R^2)}$$

we derive this equation:

$$\Delta\Delta'\cos\Sigma = \sqrt{(a^2+b^2+c^2-\sin\varrho^2)}\sqrt{(a'^2+b'^2+c'^2-\sin R^2)}$$
$$\mp \sin\varrho\sin R.$$

This being made equal to $aa'+bb'+cc'$, and the equation transformed so as to do away the sign of the square-root, we shall arrive at this expression:

$$(a^2+b^2+c^2)(a'^2+b'^2+c'^2)-(aa'+bb'+cc')^2 =$$
$$\sin\varrho^2(a'^2+b'^2+c'^2) + \sin R^2(a^2+b^2+c^2) \pm$$
$$2\sin\varrho\sin R\,(aa'+bb'+cc'),$$ or by an easy transformation,

(2)...

$$(2) \ldots (a b' - a' b)^2 + (a c' - a' c)^2 (b c' - b' c)^2 =$$
$$(a' \sin \varrho \pm a \sin R)^2 + (b' \sin \varrho \pm b \sin R)^2 + (c' \sin \varrho \pm c \sin R)^2.$$

This equation is the proper foundation of the analysis of eclipses; it is *perfectly* developed, as all the quantities which it contains refer to the centre of the earth only. It is susceptible of innumerable transformations; for either the position of the pole and the point of commencement of the angle at the same, may be arbitrarily assumed, or the sum of the three squares $e^2 + f^2 + g^2$ may be transformed into the sum of three other squares, $(e \cos a + f \sin u \,.\, \cos v - g \sin u \sin v)^2 + (e \sin u - f \cos u \,.\, \cos v + g \cos u \sin v)^2 + (f + \sin v + g \cos v)^2$, where u and v may be assumed arbitrarily.

[4.] The most simple case of an eclipse is that in which π' and R are $= 0$, or where the body eclipsed is at an infinite distance and appears as a point; this is the case of fixed stars. I shall first develop this case, and use for this purpose the 5th equation of my former paper (*Astr. Nachr.* vol. vi. No. 145. Phil. Mag. Nov. 1829, p. 338). This equation may be derived from the general equation (2) either by transferring the pole to which δ, D ... refer to the circle of declination of the fixed star at the distance of 90° from the same, and counting the angles at this pole from the star, or by writing D and A in place of the arbitrary quantities u and v of the preceding section. The latter substitution gives immediately (without applying any formulæ of spherical trigonometry),

$\sin \varrho^2 = [\cos \delta \sin (\alpha - A) - r \,.\, \cos \varphi' \sin \pi \sin (\mu - A)]^2 + [\sin \delta \cos D - \cos \delta \,.\, \sin D \cos (\alpha - A) - r \sin \pi (\sin \phi' \cos D - \cos \phi' \sin D \cos (\mu - A))]^2$, or making $\sin \varrho = k \sin \pi$ (where according to Burckhardt's tables $k = 02725$)

$$(3) \ldots k^2 = \left[\frac{\cos \delta \sin (\alpha - A)}{\sin \pi} - r \cos \phi' \sin (\mu - A)\right]^2$$
$$+ \left[\frac{\sin \delta \cos D - \cos \delta \sin D \cos (\alpha - A)}{\sin \pi} - r \Big(\sin \varphi \cos D - \cos \phi' \sin D \cos (\mu - A)\Big)\right]^2.$$

The single parts of this equation I shall denote, in the order of succession, by P, u, Q, v, so that the equation is written thus: $k^2 = (P - u)^2 + (Q - v)^2$. The latitude of the place of observation is always supposed to be known; the right ascension of the zenith μ is likewise given by the observation of the sidereal time of the immersion and emersion, and the place of the star is likewise assumed as known. But the place of the moon α, δ, her parallax π, the ratio of the equatorial radius of the earth to the radius of the moon k, and the square of the excentricity of the terrestrial meridians e^2 are not

not assumed as absolutely known, but corrections are applied to the assumed values of the same, so that $\alpha + \Delta\,\alpha$, $\delta + \Delta\,\delta$ ...denote their true values. If these corrections are supposed to be so small that their squares, products, &c. may be neglected in the alterations which $P - u$ and $Q - v$ will undergo by the substitution of the corrected values, the question is to find, by means of the equation

$$(4)\ldots(k + \Delta k)^2 = [P - u + a\Delta\alpha + b\Delta\delta + c\,\Delta\,\pi + d\Delta e^2]^2 + [Q - v + a'\,\Delta\,\alpha + b'\,\Delta\,\delta + c'\,\Delta\,\pi + d'\,\Delta\,e^2]^2,$$

the time of the first meridian which corresponds to the time of the observation, or, if the latter be denoted by t, to determine $t - d$ by an expression involving $\Delta\alpha$, $\Delta\delta$, &c. or, which is the same thing, to find the relation between d, $\Delta\,\alpha$, $\Delta\,\delta$, &c. which results from the observation. The coefficients $a, b \ldots a', b'$ are the differential quotients of $P-u$ and $Q-v$ in relation to the elements of the calculation α, δ.

[5.] Let us suppose that the required time of the first meridian, viz. the meridian whose time was employed for calculating $\alpha, \delta \ldots$ consists of two parts T and T', and let P be $= p + p'\,T'$; $Q = q + q'\,T'$, where p and q denote the values of P and Q for the time T. If the variations of P and Q were proportional to the time, p' and q' would be constants; but in reality they depend on the second and higher differences of P and Q, which, however, if T' does not exceed some hours, have only such a small effect that the variations of p' and q', compared with the variations of P and Q themselves, may be considered as quantities of a higher order. On this circumstance rests the solution of the equation (4) by successive approximations which rapidly converge to the truth. If we introduce in place of the corrections of the elements two new quantities dependent on them, so that

$$p'.\,i - q'.\,i' = a\,.\Delta\alpha + b\,.\,\Delta\delta + c\,.\,\Delta\pi + d\,.\,\Delta e^2$$

$$q'.\,i + p'.\,i' = a'.\Delta\alpha + b'.\,\Delta\delta + c'\,\Delta\,\pi + d'\,\,\Delta e^2$$

and likewise $\quad m \sin M = p - u \qquad n \sin N = p'$

$\qquad m \cos M = q - v \qquad n \cos N = q'$

the equation (4) will assume this form:

$$(k + \Delta\,k)^2 = [m\,.\cos\,(M - N) + n(T' + i)]^2 + [m \sin\,(M - N) - n\,.\,i']^2,$$

and will give, neglecting the squares of i' and Δk and introducing the auxiliary angle ψ determined by this equation $k \cos\psi = m \sin\,(M - N)$, this result:

$$T' = -\frac{m\,.\cos\,(M - N \mp \psi)}{n\,.\cos\psi} - i \mp \frac{i'}{\text{tang}\,\psi} \mp \frac{\Delta k}{n \sin\psi}$$

where, if ψ be supposed $< 180°$, the upper sign belongs to an immersion,

immersion, the lower one to an emersion; but it is more convenient to leave out the double signs and to remove the uncertainty which remains in finding ψ by its cosine by the rule, that this angle is taken in the two first quadrants if the observation is an immersion, and in the two last ones, if it is an emersion. If this rule be adopted, we have for both cases

$$(5) \ldots \mathrm{T}' = -\frac{m \cdot \cos(\mathrm{M}-\mathrm{N}-\psi)}{n \cdot \cos\psi} - i - \frac{i'}{\operatorname{tang}\psi} - \frac{\Delta k}{n \sin\psi}$$

In order to apply this solution, T′ and thence p' and q' are assumed as accurately as the existing knowledge of the difference of the meridian of the place of observation allows; the approximate determination of T′ thereby obtained, is then employed for obtaining a second approximation, and so on. I observe, however, with a view to prove the adequacy of the method, that it is not necessary to suppose an approximate knowledge of the difference of the meridians, but that in the beginning the value of T may be supposed equal to the time of the conjunction as given by the tables, this supposition being always in unison with the supposition of the convergency of the results on which the calculation is founded. When T′ has been found, T + T′ will be equal to the time of observation less the difference of meridians $t-d$, to be taken positive if easterly, consequently $d = t - \mathrm{T} - \mathrm{T}'$, or

$$(6) \ldots d = t - \mathrm{T} + \frac{m}{n} \cdot \frac{\cos(\mathrm{M}-\mathrm{N}-\psi)}{\cos\psi} + i + \frac{i'}{\operatorname{tang}\psi} + \frac{\Delta k}{n \sin\psi}$$

[6.] I shall now further develop the calculations required in the application of this method. Although it converges quickly to the result even if T′ amounts to some hours, yet the smaller the value of T′ the more convenient will be calculation, for the convergency itself will approach the nearer to its maximum, and a small T′ may be more accurately determined by its logarithm, than a greater one. It is therefore advisable to adopt for T a time which is as near to the mean of the times of the observations which are to be calculated, as this can be effected by multiples of tens of minutes of time, or generally by such parts of the day as admit of a more easy calculation from the tables or ephemerides of α, δ, π than such as are expressed by higher numbers. The sort of time in which T is to be expressed, ought to be that for which α, δ, π can be most easily found, consequently mean time, if these values are calculated from the tables, or from Encke's Ephemeris, which perfectly supplies their place; and, on the contrary, apparent time, if the columns for the moon in the *Connaissance des Tems* or the Nautical Almanac are to be applied. The values of α, δ, π are to be calculated for the

the times $T - 2^h$, $T - 1^h$, T, $T + 1^h$, $T + 2^h$...*, and from these the values of P and Q for the same moments. Let us suppose these values respectively with their successive differences to be arranged in this form:

$$\begin{array}{llllll}
T-2 & a_{\prime\prime} & & & & \\
 & & b_{\prime\prime} & & & \\
T-1 & a_{\prime} & & c_{\prime} & & \\
 & & b_{\prime} & & d_{\prime} & \\
T & a & & c & & e \\
 & & b' & & d' & \\
T+1 & a' & & c' & & \\
 & & b'' & & & \\
T+2 & a'' & & & &
\end{array}$$

If we now put $2b = b_{\prime} + b'$, $2d = d_{\prime} + d'$; then P and Q, or the parts of which they are composed, viz. p, p', q, q' become dependent on T and a, b, c ...; the formulæ by which they are expressed, shall be given when their application will be required. In this manner p' and q' are found in parts of an hour; the formulæ (5) and (6) suppose, therefore, the same unity of time. It is, however, more convenient to express d in seconds of time. This is obtained by multiplying formula (6) by such a number of seconds of that kind of time in which the observation is given, as is equivalent to the hour here assumed; this number I will generally denote by s. Then $t - T$ will be expressed in seconds of the same time, or T denotes now that time (uniformly expressed with t), for which a, b, c have been calculated. It is therefore not necessary to convert the times given by the observers into that sort of time in which T has been fixed, but the process is quite the contrary. Besides saving some trouble, we shall then, not even for converting one time into another, require any approximate value of the unknown difference of the meridians. For the sake of completeness I observe in this place that as the sidereal time, on which the u involved in the expression for u and v depends, corresponding to an observation given in solar time of an unknown meridian, cannot be calculated before the difference of meridians is known, it becomes necessary to apply to u and v small corrections of the forms $u' T'$ and $v' T'$. By this means the method directly leads likewise in this case to a result; but it will be unnecessary to dwell further on this subject, which can hardly have any practical interest.

[To be continued.]

* It would be convenient for various purposes, if a logarithmic table of interpolation similar to the one which I have given in the *Astr. Nachr.* No. 145. (Phil. Mag. Nov. 1829. p. 340.), but for intervals of 10 minutes, were inserted in the collections of astronomical tables.

XLV. *Notices respecting New Books.*

A Treatise on Poisons, in relation to Medical Jurisprudence, Physiology, and the Practice of Medicine. By ROBERT CHRISTISON, *M.D. F.R.S. E. &c. &c., Professor of Medical Jurisprudence and Police in the University of Edinburgh.* In one vol. 8vo, 698 pages, price 16s. published by Adam Black, Edinburgh, and Longman and Co. London.

WE have for some time purposed drawing the attention of our readers to this work; for though many of the details which it embraces are too professional for insertion in the Philosophical Magazine and Annals of Philosophy, it contains much chemical information, which is both new in itself and important in its applications. The object of the author is to supply a systematic work on Poisons, adapted as a guide to practitioners in medicine. Notwithstanding the great and increasing importance of the subject, this is the first original work of the kind which has appeared in the English language since the beginning of the present century; nor, considering the qualifications required for success in such an undertaking, do we feel surprise that it has not hitherto been attempted. The production of a work of this kind demands a comprehensive, and in many instances a very minute knowledge of all the principal branches of the medical profession. It requires the aid of the practice of medicine in order to distinguish the symptoms produced by poison from those of natural disease; of physiology, to explain the mode by which poisons act on the animal œconomy; and of pathology, to discriminate between the morbid appearances produced by poisonous substances and those arising from diseased action independent of poison. It is likewise necessary, as the reader of this treatise will speedily perceive, to be familiar with continental writers, not merely with the labours of the French in toxicology, but with the numerous important facts and cases recorded by the Germans,—a mine of wealth in this department, of which no previous English author has fully taken advantage. Lastly, a qualification rarely combined with learning and experience in medicine—a knowledge of chemistry—is essential. We do not mean such theoretical knowledge as suffices to understand a chemical work; but that combination of sound theory and manual skill, which enables the possessor to detect the real fallacies of a process, and to devise and perform others which are effective. The praise of having brought these qualifications to bear on his subject may justly be awarded to Dr. Christison; and, besides great industry in collecting the opinions and facts of others, he may claim the merit of contributing much original matter of his own. His style is clear and unaffected; and his classification and mode of treating his subject are guided by that practical good sense which renders the work convenient and agreeable for consultation. In fact, Dr. Christison has supplied a work that was much wanted, and has performed it in a manner which reflects credit both on himself and on the University with which he is connected.

Dr.

Dr. Christison has divided his work into two principal parts; the first of which treats of poisoning in general, and the second of individual poisons. His object in the former, is to consider the physiological action of poisons, and to lay down those principles relative to the management of cases of poisoning, which apply generally to the subject. The individual poisons are considered under the three classes of *Irritants*, *Narcotics*, and *Narcotico-acrids*. The first divison comprehends those poisons, the principal action of which is to excite inflammation in the parts where they are applied; the second, such as act solely or chiefly by producing disorder in the nervous system; and the class of narcotico-acrids includes those which possess a double action, the one local and irritating, and the other remote, consisting of an impression on the nervous system. The subjects discussed in the first part are too exclusively medical for our pages: we shall extract from the history of the individual poisons such portions as will serve both as specimens of the work, and convey useful, and in general new information to the reader.

Arsenic.—As very incorrect notions prevail respecting the taste of arsenious acid, we extract Dr. Christison's remarks on this subject. "It has long been almost universally believed to be acrid, and is described to be so in most systematic works, and in many express treatises: but in reality it has little or no taste at all. The reader will find some details on this point in a paper I published lately in the Edinburgh Medical and Surgical Journal. In the present work it is sufficient to observe, that I have repeatedly made the trial, and seen it made by my request by several of my scientific friends; and that after continuing the experiment as long and extending the poison as far back as we thought safe, we all agreed that it had hardly any taste at all,—perhaps towards the close a very faint sweetish taste. I have hitherto found only one authority who has made the observation that arsenic has no taste,—namely, Dr. Addington, the chief Crown witness on the trial of Miss Blandy; a few others, and more particularly Hahnemann, Dr. Gordon, and Mr. Walker, a witness on a late trial, have said that it is sweet; but all the other authors I have consulted mention that it is acrid, and one of these, Professor Fodéré, even says that a grain causes an indescribable and insupportable metallic taste. It is impossible to make with safety satisfactory experiments on its taste, when it reaches the back of the palate; but we may rest assured that it often makes no impression at all, as it has been swallowed unknowingly with articles of food. This fact it is essential to remember, as many ignorantly believe that when swallowed, even in moderate quantity, it must cause a sense of acridity. I have not been able to find any actual case where this sensation was perceived; and it is therefore probable that the mistake, which the present remarks are intended to rectify, has arisen from the impression in the act of swallowing having been confounded with the inflammation in the throat subsequently developed along with the other inflammatory symptoms. And so Navier remarked, that the solution has at first

a bland

a bland taste like milk, but in a few minutes excited a sense of roughness (*âpreté*), and soon after the usual effects of burning."

We need not stop to describe Dr. Christison's method of detecting the presence of arsenic, as his views are already before the public. On the odour of arsenic, as a test, we may, however, extract the following comment. "This test should be altogether discarded. It does not always detect arsenic when present; and the alliaceous odour is not an infallible proof that it is present. Zinc powder projected on burning fuel emits the same odour. Phosphorus, phosphoric acid, and the phosphates give out a similar odour; and I have frequently remarked one very like it from burning paper. What is of more consequence, however, a very small portion of vegetable or animal matter, present in the matter subjected to trial, obscures the alliaceous smell entirely. This I have often observed, and the same thing was stated long ago by Pyl and Hahnemann. If any one should nevertheless wish to have recourse to this test, the proper way to try it, is to breathe gently with the nostrils into the tube immediately after the metal has been sublimed, and then to smell it."

In speaking of the detection of arsenic by means of the ammoniacal nitrate of silver, and of the obscurity occasioned by the presence of sea-salt, the following simple method is given for avoiding uncertainty arising from this source. "The best way of getting rid of this difficulty is to use in the first instance, not the ammoniacal nitrate, but the nitrate of silver as long as any white precipitate falls down, to add a slight excess of that test, and then, after subsidence, to drop in ammonia. No arsenic is thrown down by the first steps of this process; but if any be present, it is thrown down in the form of the rich yellow arsenite of silver, on the subsequent addition of ammonia. This very simple mode of getting rid of the chloride of sodium has been lately suggested by Dr. Forbes, Professor of Chemistry, Aberdeen." The same principle has been since applied by Dr. Christison, for removing animal substances, as preparatory to the precipitation of arsenic by sulphuretted hydrogen; a preparation which should never be omitted when much animal matter is present, as it is very apt to adhere to the sulphuret of arsenic, and prove troublesome in the process of reduction. This difficulty is avoided by adding nitrate of silver as long as any precipitate falls, when all the animal matter is thrown down in combination with oxide of silver, and arsenious acid remains in solution, provided care is taken that no uncombined alkali is present. The excess of silver is subsequently removed by muriate of soda, and the arsenic thrown down as usual by sulphuretted hydrogen. It is important, however, to neutralize the free nitric acid before transmitting sulphuretted hydrogen gas through the liquid; since otherwise the sulphuret of arsenic would be mixed with free sulphur, the presence of which renders the reduction by black flux both difficult and uncertain.

As the action of water on orpiment is important and not generally noticed in systematic works on chemistry, we extract the following

following passage. "It has been stated by Hahnemann in his elaborate work on arsenic, that the pure sulphurets are somewhat soluble in water,—that the native orpiment is soluble in 5000 parts of water with the aid of ebullition, and that the artificial orpiment by precipitation is soluble in 600 parts of water. Hahnemann, however, was mistaken in supposing that the water dissolved these sulphurets. It does not dissolve, but decomposes them. Very lately M. Decourdemanche has found that, by slow action in cold water, and much more quickly with the aid of heat, the arsenical sulphuret is decomposed by virtue of a simultaneous decomposition of the water, sulphuretted hydrogen being evolved, and an oxide of arsenic remaining in solution. And he has further remarked, that this change is promoted by the presence of animal and vegetable principles dissolved in the water."

Much difference of opinion has prevailed regarding the influence of arsenic over the putrefaction of the bodies of persons who have been poisoned with it. Till lately the prevailing opinion was, that the putrefactive process is accelerated; and in some instances this really appears to have been the case. But others maintain that arsenic sometimes proves powerfully antiseptic; and several remarkable cases, in proof of this opinion, are quoted by Dr. Christison from German writers, by whom the subject has of late been much discussed. That arsenic acts as an antiseptic on the parts with which it is in contact, is clearly established by the following evidence:—

"Arsenic is a good preservative of animal textures when it is directly applied to them in sufficient quantity. This is well known to stuffers of birds and beasts, was experimentally ascertained by Guyton Morveau, and has also come under my own observation. I have kept a bit of an ox's stomach four years in a solution of arsenic, and, except slight shrivelling and whitening, I could not observe any change produced in it."

"Dr. Kelch of Königsberg, buried the internal organs of a man who had died of arsenic, and whose body had remained without burial till the external parts had begun to decay; and on examining the stomach and intestines five months after, he found that the hamper in which they were contained was very rotten; but that 'they had a peculiar smell, quite different from that of putrid bowels, were not yet acted on by putrefaction, but as fresh as when first taken from the body, and might have served to make preparations. They had lost nothing of their colour, glimmer, or firmness. The inflamed spots on the stomach had not disappeared; and the small intestines also showed in some places the inflammatory redness unaltered.'"

"In the case of the girl Warden, which has been several times alluded to, the internal organs were also preserved somewhat in the same manner. The body had been buried three weeks; yet the mucous coat of the stomach and intestines, except on its mere surface, was very firm, and all the morbid appearances were consequently quite distinct. Nay, three weeks after disinterment, except

cept that the vascularity had disappeared, the membranes and the appearances in them remained in the same state. A similar case has been recorded by Metzger. It is that of an old man who died after six hours' illness, and in whose stomach three drachms of arsenic were found. The body had been kept ten days in February before burial, and was disinterred eight days after that; yet there was not the slightest sign of putrefaction anywhere. A parallel case was described by myself in the Edinburgh Medico-Chirurgical Transactions."

In a case of poisoning with arsenic examined by Dr. Borges, Medical Inspector at Minden, fourteen weeks after death, "the stomach and intestines were firm, of a grayish-white colour, and contained evident crumbs of bread, while all the other organs in the belly were pulpy, and the external parts adipocirous." So also in a case which happened at Chemnitz in 1726, "the skin was everywhere very putrid, but the stomach and intestines were perfectly fresh. In the case of Warden, the appearances were precisely the same. Three weeks after burial the Dundee inspectors found the external parts much decayed; and three weeks later, the stomach and intestines were found by myself in a state of almost perfect preservation. A striking experiment performed by Dr. Borges on a rabbit will likewise illustrate admirably the fact now under consideration. The rabbit was killed in less than a day with ten grains of arsenic, and its body was buried for thirteen months in a moist place under the eaves of a house. At the end of this period it was found that the skin, muscles, cellular tissue, ligaments, and all the viscera, except the alimentary canal, had disappeared, without leaving a trace; but the alimentary canal, from the throat to the anus, along with the hair and the bare bones, was quite entire."

But it is also maintained by some German writers that the preservative influence of arsenic is not confined to the parts actually in contact with it, but sometimes extends to the whole body; and Dr. Christison has quoted instances of the whole body, in animals poisoned with arsenic, manifesting an unusually small tendency to decay. But this point is not decided. The facts already mentioned show that the body often decays as usual, while the parts to which arsenic had been actually applied are preserved; nor are those cases, in which the whole body remained entire, sufficiently numerous to prove that the preservation was really produced by arsenic, and not by accidental extraneous causes. Dr. Christison closes his account of the subject with the following remarks:—"Whatever credit is given to the opinion of the German medical jurists, in favour of the preservative power of arsenic, the English medical jurist will not lose sight of the fact, that in many instances the body in this kind of poisoning has been found long after death in so perfect a state as to admit of an accurate medico-legal inspection, and a successful chemical analysis. In one of his cases Dr. Bachmann detected arsenic in the stomach fourteen months after interment; and Dr. Borges had no difficulty in detecting it in an animal after thirteen months."

Mercury.

Mercury.—Dr. Christison introduces his remarks on the detection of corrosive sublimate in mixed fluids, by referring to the experiments of Berthollet, Orfila, Boullay, and Taddei, on the decomposition of this poison by many organic substances. It is now ascertained that various vegetable solutions convert corrosive sublimate into calomel, some producing the change rapidly, and others after an interval of hours or even days. Strong tea immediately becomes turbid from this cause, when mixed with a few grains of the poison, while infusion of galls acts only after the space of some hours. Many animal principles produce the same change, some acting instantaneously, and others slowly. Of the soluble animal substances, albumen, curd, osmazome, and gelatine, possess this property in an eminent degree, especially the former, which is hence employed as an antidote to the effects of corrosive sublimate. Fibrin, coagulated albumen, membranous surfaces, and generally, the soft solids both in the dead and living animal, produce a similar effect; and, in fact, the corrosive power of this poison depends on the chemical action here alluded to. Owing to this cause, death may be occasioned by corrosive sublimate, and yet none of the poison be found either in the stomach or in the substances ejected from it. The chemist has therefore to search for decomposed as well as undecomposed poison, the former existing in the state of calomel in combination with organic matter.

We have premised these particulars in explanation of the process recommended by Dr. Christison in cases of poisoning by corrosive sublimate, in all of which cases the poison is necessarily more or less subject to the decomposing agency of organic substances. Dr. C. remarks that he "has experienced considerable difficulty in fixing on a satisfactory process for detecting corrosive sublimate, when it exists in mixtures of the kind now described, and in such minute proportions as the medical jurist will generally have to search for. I have satisfied myself that none of the methods in common use are sufficient in these circumstances;—even the otherwise well-designed processes of Professor Orfila being in some essential respects defective. On the whole, the following plan has appeared to me the most simple and the most generally applicable. It is a double process, of which sometimes the first part, sometimes the second, sometimes both, may be required. The first removes the corrosive sublimate undecomposed from the mixture, which may be accomplished when its proportion is not minute; the second, when the proportion of corrosive sublimate is too small to admit of being so removed, separates from the mixture metallic mercury; and the analyst will know which of the two to employ by using protochloride of tin as a trial test in the following manner."

"A fluid mixture being in the first instance made, if necessary, by dividing all soft solids into small fragments, and boiling the mass in distilled water, a small portion is to be filtered for the trial. If protochloride of tin causes a pretty deep ash-gray or grayish-black colour, the first process will probably be successful; if the shade

required is not deep, that process may be neglected, and the second put in practice at once."

First branch of the process.—In order to remove the corrosive sublimate undecomposed, the mixture, without filtration, is to be agitated for a few minutes with about a fourth part of its volume of sulphuric æther; which possesses the property of abstracting the salt from its aqueous solution. On remaining at rest for half a minute or a little more, the æthereal solution rises to the surface, and may then be removed with a pipette. It is next to be filtered if requisite, evaporated to dryness, and the residue treated with boiling water; upon which a solution is procured that will present the properties characteristic of corrosive sublimate in its dissolved state.

Second branch of the process.—If the preceding method should fail, or shall have been judged inapplicable, the mixture is to be treated in the following manner. In the first place, all particles of seeds, leaves, and other fibrous matter of a vegetable nature, are to be removed as carefully as possible. This being done, the mixture, without undergoing filtration, is to be treated with protochloride of tin as long as any precipitate or coagulum is formed. This precipitate, even if it contains but a very minute proportion of mercury, will have a slate-gray tint. It is to be collected, washed, and drained on a filter; from which it is then to be removed without being dried; and care should be taken not to tear away with it any fibres of the paper, as these would obstruct the succeeding operations.

"The precipitate is next to be boiled in a moderately strong solution of caustic potash contained in a glass flask, or still better in a smooth porcelain vessel glazed with porcelain; and the ebullition is to be continued till all the lumps disappear. The animal and vegetable matter will thus be dissolved; and on the solution being allowed to remain at rest, a heavy grayish-black powder will begin to fall down in a few seconds. This is chiefly metallic mercury, of which, indeed, globules may sometimes be discerned with the naked eye, or with a small magnifyer." The powder should then be washed and sublimed in a tube, so as to obtain the metal in a perfectly pure and characteristic state.

"The second branch of this process is very delicate. I have detected by it a quarter of a grain of corrosive sublimate mixed with two ounces of beef, or with five ounces of new milk or porter, or tea made with a liberal allowance of cream and sugar. I have also detected a tenth part of a grain in four ounces of the last mixture, that is, in 19·200 times its weight."

In a subsequent Number, we shall extract Dr. Christison's account of the action of natural waters on lead.

A Supplement to "The Amateur's Perspective." By Richard Davenport, Esq. London, 1829; 4to. pp. 64.

It is probable that we have now a far greater number of amateur draughtsmen than any age has known,—naval, military and private tourists,

tourists, male and female; and their published travels contain an immensely greater display of sketches and descriptive plates than heretofore; and their private portfolios, of elegant drawings. The utility of this practice is not ill appreciated in society; and the pleasure it affords to the tourist and to the reader is very generally acknowledged. It is therefore somewhat surprising that the study of PERSPECTIVE should prevail so little. Is it the fault of the books, the teachers, or the scholars? Is it the fear of the undertaking, or want of perception of its value, that indisposes amateurs to undertake this study? It must be acknowledged that some of our first masters have been careless of Perspective up to a certain degree; but the neglect appears in general in the subordinate parts only of their pictures: still their inattention, even in this degree, has left an example, which, being on the indolent side, is too readily followed by artists of inferior talent; and it deceives the eye and habituates it to incorrect representation.

Notwithstanding, however, this unperceived morbid habit, good Perspective is always pleasing. The untutored eye catches the effect, and perceives unexpectedly the resemblance to nature. Who is not delighted by the magic of the Dioramas? But (without more than mentioning those specimens of the art) who does not feel pleased with a true representation of even the simplest scene, where the surface of a road-side pond appears level and flat,—where the street in a country town appears to open to the spectator as though he could trot along the hard ground where the boy's hoop seems balanced as it runs on the pavement? While, on the other hand, however finely a picture may be coloured, or however beautifully pencilled a drawing, how lost is the effect when the artist has not taken the pains to consider where his horizontal line is, or has not known to what points his vanishing lines should tend! The spectator feels at once, though he may not know why, that the figures appear not to stand on the ground;—that the two sides, whether of a cathedral or of a cottage, look like one long face;—that the water slopes;—or that even a round tub appears bigger on one side than the other.

PERSPECTIVE is the first requisite—the *sine quâ non* of picture. Picture is the representation of *space* and *bulk* on *superficies*. It consists in *form, light and shade,* and *colour*; but form comes first,—DRAWING. Now the form of one and the same object varies infinitely, in the infinite variety of positions in which it is viewed; and the representation of that form, *according to that position*, is PERSPECTIVE; which, therefore, is the first essential of picture.

The work to which the publication before us is called a Supplement, was noticed in the Phil. Mag. and Annals, N. S. vol. v. p. 127, with extracts from the Preface, giving an idea of its nature and design, together with its table of contents. A main part of the professed intention of the work appears to be to encourage the study of Perspective, by showing,—that previous knowledge of geometry is not necessary to convince amateurs that the study requires no more knowledge or talent than is necessary to one who studies a map, tracing positions by the lines of longitude and latitude; and that not

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only the *effect* of their sketches but the facility of the *time* saved in taking them, will soon overpay the time employed in acquiring the theory and the practice;—that the hours spent in the study will soon be compensated by the hours saved in the field; by means of the possession of principles, and the knowing what *to aim at.* In the conclusion of the former part, the author expressed himself as unwillingly breaking off, while some parts of his subject remained but partially treated of; leaving the intention of continuing the work, dependent on the reception his volume should meet with.

In the Introduction to the present Supplement, he says, "Without waiting for the result, he has considered such parts to be essential to the theory, and the work incomplete in its plan without the introduction of them."

The Supplement begins with the FIELD OF VISION, of which the author gives a more complete theory than in the first volume, stating more particularly the *ground* of its limits; and that those limits do not subsist where other writers have placed them;—showing also why pictures, when reduced for the purpose of engraving, or otherwise, will not produce the same perspective effect in the larger originals, although the same proportions may be exactly maintained; and showing also why, in small diagrams, the Perspective, though true, will strike the eye as distorted. In the chapter on the *Horizon-line,* he examines and in some measure opposes the theories of former writers, and maintains that the height of the horizon in the picture "has no more to do with the height or width of the picture, than it has with the thickness of the wood-frame in which the picture is placed." He then illustrates his theory in a very clear manner, by a series of wood-cuts, representing sea and ships with the same outline and on the same scale, but as viewed from different heights, and therefore with different positions of the horizon-line; adopting a new term, and making a distinction between a *high horizon* and an enlarged *sub-horizon* or low base; and *vice versâ:* and this hitherto irregularly discussed theory, he appears to us to have placed on a footing from which it is not likely to be removed. Mr. Davenport afterwards contends, against the authority of almost all his predecessors on this subject, that the PRIME VERTICAL line must be placed equally distant between the two sides of the picture; and gives his reasons why the "prime vertical must cut the horizon-line in its centre, though it is not necessary that it should itself be intersected by that line in its own centre." He endeavours to give a philosophical rationale of the causes of these and other apparent anomalies, and of the difficulties and deceptions that occur in pictorial representation; such as the difficulty of *down-hill* views;—the deceptive magnitudes of objects on very high edifices, as compared with those at equal horizontal distances; and, contrary to the most generally received estimation, criticizes what he calls the deceptive *dis*-proportions of St. Peter's at Rome. He also gives some further problems principally for the finding of distant vanishing points, and notices the effect of the *refraction* of the atmosphere, of *reflection* from the surface of water, &c.; but we think that some examples

both

both of reflections and shadows, and some problems in *inverse* perspective (i. e. the making out the geometric form from the perspective one, as well as the perspective from the geometric form), would be desirable additions; for although it is divided into chapters, with a title to each, and a running title at the top of the page, yet from the necessary connection of the heads of the theory, reserved for this volume, the details of the subject are introduced more than once in some different points of view, and the reference from one to the other is not so easy as it ought to be.

We subjoin, from the Preface, some remarks on the scientific character of the late Dr. Wollaston, which will, we think, be interesting to our readers:

"In a paragraph on mechanical aids, the inventor of the *Camera Lucida* was alluded to in friendly jocularity. No recollection of him can now occur without lament. A copy of the book was sent to him from the country by the Author, who anticipated a smile at their next meeting; useful criticisms on the treatise, and assistance in the part that remains. Indeed, one motive the Author had for pausing where he did, was the hope of being thus furnished with more concise demonstrations, and happier illustration of his theory. Alas! the days of his friend's smiles were past, and the hours that remained were too valuable to be employed in the perusal of a work of such minor importance. He is no more. He has vacated a department in the philosophical world, which probably no man living can supply.

"There may be greater astronomers,—greater mathematicians,—greater chemists,—geologists,—botanists,—more able and practical mechanics—more experienced physicians;—but the astronomer,—the mathematician,—the optician,—the botanist,—the chemist,—the physician,—the natural philosopher,—the civil engineer,—the mechanic (of every description, maker of steam-engines, chronometers, telescopes, barometers, hydrometers),—the metallurgist,—the framer of every useful invention, down to the manufacturer of caoutchouc-cloth, all derived assistance from him. He stood as interpreter between the sciences, and enabled each one to give its aid to the others.

"In generalization of knowledge,—minute precision in the observation of phænomena,—in almost undeviating sagacity in investigation,—inexhaustible resource in his adaptation of means, and ingenuity of contrivance,—in quickness and universality in the application of analogies,—who can compete with WOLLASTON?"

A table of contents, or rather an index, would improve this Supplement, and will, we hope, be added to a future edition.

The First Book of Euclid's Elements. With Alterations and Familiar Notes. Being an attempt to get rid of Axioms altogether; and to establish the Theory of Parallel Lines, without the introduction of any principle not common to other parts of the Elements. Third Edition. By Lieut. Col. THOMPSON, *F.R.S.* London, Ridgway. *Sewed.*

Of the Axioms, it is stated in the Preface, that "some have been demonstrated as Theorems; others, resolved into the Definitions of the

the things concerned; and others, into Corollaries from the rest. That which declared that the whole is greater than its part, has been omitted as amounting only to an identical proposition, that the greatest is greatest.' And on the attempt to solve the *vexata questio* of Parallel Lines, it is added, that 'the use made of motion or moving magnitudes in the 11th and 12th Books of Euclid, has been considered as sufficient for the introduction of a straight line that moves along another straight line keeping ever at right angles to it; without infringement of the assertion in the title-page, that no new principle has been introduced. But if this should not be conceded, the imputation is still only on the correctness of the title-page.'

The process by which the establishment of the Theory of Parallel Lines is sought, is by demonstrating that if at the extremities of any straight line, two other straight lines be drawn towards the same side, equal to one another and making equal angles with the first straight line or *base*, and their extremities be joined, the angles opposite to the base shall be equal to one another, and the side opposite to the base shall be parallel to the base; and subsequently endeavouring to prove, that if the angles at the base are right angles, the angles opposite to the base shall also be right angles. After which, it would be easy to establish the equality of the three angles of a triangle to two right angles, and all the properties of parallel lines.

The way in which this demonstration is pursued, is by trying to establish, first, That if the equal angles at the base of the quadrilateral figure are greater than right angles, the angles opposite to the base shall be less. And this is done, by placing a number of such quadrilateral figures side by side so that their bases join, and proving that if these bases be severally produced, they shall cut off greater and greater portions in succession from the side of the first quadrilateral figure, and consequently, if the number of quadrilateral figures be increased, some of them shall meet the series of lines formed by the sides of the quadrilateral figures which are opposite to their bases; from which, (and its having been previously established that in each of the quadrilateral figures the side opposite to the base is parallel to the base,) it is inferred that the sides opposite to the bases of the quadrilateral figures are not in one straight line, but make with each other an angle that is less than two right angles.

It will be perceived that this contains the principle which was brought forward by M. Legendre in the 7th Edition of the '*Eléments de Géométrie*,' for the purpose of proving that the three angles of a triangle cannot be *greater* than two right angles, and was subsequently withdrawn in consequence of the imperfection of the proof of the other point required, viz. that they cannot be *less*. The only difference is, that the principle was there applied to triangles; and here it is applied to quadrilateral figures. The coincidence is noted in the Preface; but with an intimation that the application to quadrilateral figures was completed many years before the author had the opportunity of being acquainted with the other demonstration. And as the proof of this particular portion of the subject has been for a long

long time before the public without being called in question, (the publication by M. Legendre being believed to have been as early as 1815,) it may probably be set down as what there is no reason to dispute.

What is left then to be determined, is whether the last author has been successful in getting over the remaining step; or proving that if the equal angles at the base of the quadrilateral figure are *less* than right angles, the angles opposite to the base (provision always included that the sides do not meet) shall be *greater* than right angles.

With this view, his proceeding is directed to establish, that if two equal finite straight lines terminated in the same point, make any angle that is less than the sum of two right angles, and this angle be bisected by a straight line of unlimited length which may be called the *axis;* and at the outward extremity of each of the two equal finite straight lines be added another straight line equal to the first, having its extremity also terminated in the extremity of the other, and making with it an angle equal to the first-mentioned angle, and on the same side of the line; and at the outward extremity of each of these, be added another straight line as before, and so on continually; and the extremities of every two equal straight lines so successively added at one and the same time at the two ends of the series, be joined by a straight line or *chord;* each of these chords shall make the angles at the two *cusps,* where it meets the equal straight lines, equal to one another; and (so long as none of the equal straight lines meets the axis) the several chords shall in succession make greater and greater angles at the cusp, each than the preceding. This is done by the help of the inferences previously established with respect to quadrilateral figures, and by joining the extremity of one chord cross-wise with the extremity of the next, so as to make one zigzag line which demonstrates almost by inspection the successive enlargement of the angles. This is followed by a Scholium, warning against the mistake of supposing that because it has been proved that the angle will continually increase, it can therefore be concluded that it will ever arrive at a certain specified magnitude; for evidence of which, reference is made in the Notes, to the well known instance of the series $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+$ &c., in which the magnitude of the sum goes on perpetually increasing, and yet never arrives at being equal to double the first term. But this is stated to form no objection to examining the consequences which must result, *if* the angle is ever found to arrive at a certain magnitude that may be specified.

The next Proposition is to prove, that in a series of equal straight lines like the last-mentioned, if the angle at the cusp be ever equal to, or greater than, half the angle made by the two first equal straight lines; the angular points shall lie in the circumference of a circle, whose centre is in the axis, in the part of it which is cut off by the chord; and the series, being continued, shall at length meet the axis. The demonstration, though lengthy when given with all the forms, is such as will be easily supplied by any person in the habit of geometrical solutions.

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The next step is directed to demonstrate, that in a series of equal straight lines as before, if a straight line of unlimited length both ways, be moved along the axis in the direction which removes it from the vertex, keeping ever at right angles to the axis; such straight line shall never quit or cease to meet the series, without the series having previously met the axis. And the proof is rested on the fact, that whenever this moving straight line passes through the extremities of two of the equal straight lines of the series, there must always be two more of the equal straight lines, making a *smaller* angle with the chord on the side which is towards the axis, than in the case that last preceded; and consequently the only possibility there is for the moving line escaping from the series by finding no more of the equal straight lines over which to pass, is by some of these equal straight lines either making no angle at all with the last chord, or falling on the other side of it. But to do either of these, the angle at the cusp must previously have exceeded the magnitude which has been held forward as involving the necessity of the angular points lying in the circumference of a circle, whose centre is in the axis at a point between the travelling line and the vertex. And as this travelling line cannot have quitted or ceased to meet the series *before* it has passed the point in which the series meets the axis; it is concluded that if it be ever found to have quitted or ceased to meet the series, the series must have *previously* met the axis.

To these preliminaries, succeeds the Proposition, that if the angles at the base of the quadrilateral figure be *less* than right angles, the angles opposite to the base shall be *greater* than right angles. And the process for proving this, is by taking a quadrilateral figure of the kind in question, producing one of its equal sides in the direction opposite to the base for an axis, and placing on each side of it a number of such quadrilateral figures side by side, whose bases will form a series of the nature described in the previous Propositions. After which, the proceedings are directed to establish, that if a straight line of unlimited length be supposed to move along the axis till it cuts it in a point beyond the side of the quadrilateral figure which was produced to make the axis, and thence be still further moved forward in the same direction, it must do *one of three things*. It must either fall in with two of the angular points of the series, and there make an angle at the cusp *less* than half the angle at the vertex or than one of the angles at the base of one of the quadrilateral figures; or it must make an angle at the cusp *equal to or greater than* this angle; or it must be found to have ceased to meet the series altogether, in which event it has been shown that the series must have previously met the axis. In the first of these cases, the demonstration is directed to show, that the angles opposite to the bases of the quadrilateral figures cannot be right angles, because then the sides opposite to the bases would be in one straight line, and there would be two straight lines at right angles to the axis, meeting each other, which is impossible; and that they cannot be less than right angles, because then they must all lie on the other side of a perpendicular to the axis, and *à fortiori* could never meet the other line. And in the other two cases, reference

ence is made to the preceding demonstrations to show, that the series being continued must meet the axis, and the angular points lie in the circumference of a circle; whence it is inferred, that the sides opposite to the bases of the quadrilateral figures must also meet the axis, and form an interior polygon to the series formed by the bases. From which it is concluded, that these sides cannot make with each other, angles equal to two right angles, for then they would be in one straight line, and this straight line, with the axis, would inclose a space; and, *à fortiori*, as before, they cannot make angles less than two right angles; wherefore they must make greater, or the angles of the quadrilateral figures which are opposite to their bases must be each greater than a right angle. In a Note, some reasons are advanced for believing, that the third of these cases, or that where the moving line is supposed to quit the series, might be got rid of by demonstrating, that the moving line must always pass through at least *one* pair of cusps after passing beyond the extremity of the side of the quadrilateral figure which was produced to make the axis; and consequently that the other two cases may be made to include all that in reality are possible.

If all this is considered as established, it is manifestly an easy step to the demonstration that if the equal angles at the base of the quadrilateral figure are right angles, the angles opposite to the base must be also right angles; and thence, that the side opposite to the base must be equal to the base. After this it may be shown, that the angles of any right-angled triangle must be equal to two right angles; by completing a quadrilateral figure, which shall have the angles at the base right angles. And the same may be proved in any other triangle, by selecting a side that lies between two acute angles, and drawing a perpendicular to it from the angular point opposite; which will divide the triangle into two right-angled triangles.

If all this is supposed to hold good, the Proposition on Parallel Lines commonly known as the Twelth Axiom, may be established in the case where one of the angles is a right angle, by drawing a perpendicular from any point in the line which makes the angle less than a right angle, to the line which intersects the two; and thence establishing successive ranks of quadrilateral figures, of which all the opposite sides may, from the past Propositions, be proved equal, and all the angles right angles; and showing that their diagonals taken in an oblique succession shall all lie in one straight line, and if the number of quadrilateral figures is continued, shall of necessity cut the other line. After which, the remaining case, or that where neither of the angles is a right angle, may be solved by the assistance of the first.

As is intimated in the Preface, it is probable that these demonstrations, if no flagrant fallacy should be detected in them, may be found capable of concentration and abridgement.

XLVI. *Proceedings of Learned Societies.*

ASTRONOMICAL SOCIETY.

April 7.—THE following communications were read:—1. Observations of the Planets made at the Imperial Observatory of Vienna, in the year 1828, by J. J. Littrow.

2. Observations of Occultations of Stars by the Moon, at Mr. South's Observatory, Kensington.

3. *Extract of a letter from Charles Perkins, Esq. to the President:—*

"I observed most of the occultations last night (March 28). My friend Mr. Holland was with me, and we both considered the stars 99 Tauri, and Piazzi IV. 102, to have been visible on the moon's disc. Each observer used a 42-inch achromatic with $2\frac{3}{4}$ aperture, mine with a power of 120, and Mr. Holland tried one of his own eye-pieces, the power of which he estimated at 336. With this he observed the star distinctly, and the moon's dark limb well defined."

4. On a method of ascertaining any inaccuracy in the formation of the Pivots of Transit Instruments, &c., or any subsequent derangement in their shape. By Lieut. Peter Lecount, R.N.

5. "Fourth Series of Observations with a 20-feet Reflector, containing the mean places and other particulars of 1236 Double Stars, as determined at Slough in the years 1828 and 1829 with that instrument (the greater part of them not previously described). By J. F. W. Herschel, Esq."

GEOGRAPHICAL SOCIETY OF LONDON.

A numerous Meeting of the Members of the Raleigh Travellers' Club, and other gentlemen, was held at the Thatched House, on Monday, the 24th of May, John Barrow, Esq., in the Chair; when it was submitted,—That, among the numerous literary and scientific societies established in the British metropolis, one was still wanting to complete the circle of scientific institutions, whose sole object should be the promotion and diffusion of that most important, useful, and entertaining branch of knowledge, Geography.

That a new and useful Society might therefore be formed, under the name of "The Geographical Society of London."

That the interest excited by this department of science is universally felt; that its advantages are of the first importance to mankind in general, and paramount to the welfare of a maritime nation, like Great Britain, with its numerous and extensive foreign possessions.

That its decided utility in conferring just and distinct notions of the physical and political relations of our globe must be obvious to every one; and is the more enhanced by this species of knowledge being attainable without much difficulty, while at the same time it affords a copious source of rational amusement.

That although there is a vast store of geographical information existing in Great Britain, yet it is so scattered and dispersed, either in large books that are not generally accessible, or in the bureaus of the public

public departments, or in the possession of private individuals, as to be nearly unavailable to the public.

The objects, then, of such a Society as is now suggested would be,

1. To collect, register, and digest, and to print for the use of the Members, and the public at large, in a cheap form and at certain intervals, such new, interesting, and useful facts and discoveries as the Society may have in its possession, and may from time to time acquire.

2. To accumulate gradually a library of the best books on geography—a selection of the best Voyages and Travels—a complete collection of maps and charts, from the earliest period of rude geographical delineations to the most improved of the present time; as well as all such documents and materials as may convey the best information to persons intending to visit foreign countries; it being of the greatest utility to a traveller to be aware, previously to his setting out, of what has been already done, and what is still wanting, in the countries he may intend to visit.

3. To procure specimens of such instruments as experience has shown to be most useful, and best adapted to the compendious stock of a traveller, by consulting which, he may make himself familiar with their use.

4. To prepare brief instructions for such as are setting out on their travels; pointing out the parts most desirable to be visited; the best and most practicable means of proceeding thither; the researches most essential to make; phænomena to be observed; the subjects of natural history most desirable to be procured; and to obtain all such information as may tend to the extension of our geographical knowledge. And it is hoped that the Society may ultimately be enabled, from its funds, to render pecuniary assistance to such travellers as may require it, in order to facilitate the attainment of some particular object of research.

5. To correspond with similar societies that may be established in different parts of the world; with foreign individuals engaged in geographical pursuits, and with the most intelligent British residents in the various remote settlements of the empire.

6. To open a communication with all those philosophical and literary societies with which Geography is connected; for as all are fellow-labourers in the different departments of the same vineyard, their united efforts cannot fail mutually to assist each other.

The Meeting then proceeded to nominate a Provisional Committee to consider and propose resolutions to be submitted to a General Meeting.

July 16.—At a Meeting of the above Society, held at the Rooms of the Horticultural Society, Regent-street, on Friday the 16th July, J. Barrow, Esq. in the chair; the following Resolutions were adopted:—

1. That the Society be called "The Geographical Society of London."

2. That the number of ordinary Members be not limited; but that the number of Honorary Foreign Members be limited, as shall hereafter be determined.

 3. That

3. That the Council of the Society consist of a President, four Vice-Presidents, a Treasurer, two Secretaries, and twenty-one other Members, to conduct the affairs of the Society.

4. That the election of the said Council and Officers be annual.

5. That the office of President be not held by the same individual for a longer period than two consecutive years, but that he be eligible for re-election after the lapse of one year.

6. That one of the four Vice-Presidents go out annually; he being eligible, however, for re-election after the lapse of one year: but the Treasurer and Secretaries may be annually re-elected.

7. That seven of the twenty-one other Members constituting the Council, go out annually, at the period of the general election of the Officers of the Society.

8. That the Admission Fee of Members be 3*l.*, and the Annual Subscription 2*l.*; or both may be compounded for by one payment of 20*l.*

9. That such part of the Funds of the Society as may not be required for current expenses, be placed in the public securities, and vested in the names of three Trustees, to be hereafter appointed by the President and Council.

10. That these three Trustees be Supernumerary Members of the Council.

11. That early in November next a General Meeting be held, to decide on a Code of Regulations and By-Laws, for the management of the Society, which the President and Council will, in the mean time, prepare, to be submitted to the said Meeting.

12. And lastly, That the following Noblemen and Gentlemen compose the Council and Officers of the Society for the first Year—

President: The Right Honourable Viscount Goderich, F.R.S.—*Vice Presidents:* John Barrow, Esq., F.R.S.; Lieut.-Col. Leake, F.R.S.; G. Bellas Greenough, Esq., F.R.S.; Sir J. Franklin, F.R.S.—*Treasurer:* John Biddulph, Esq., F.H.S.—*Secretaries:* Captain M'Konochie, R.N.; Rev. George C. Renouard, *Foreign and Hon. Sec.*—*Council:* Viscount Althorp, F.R.S.; Francis Baily, Esq., F.R.S.; Captain Beaufort, R.N., F.R.S.; John Britton, Esq., F.S.A.; W. Brockedon, Esq.; Robert Brown, Esq., F.R.S.; Sir A. de Capell Brooke, Bart., F.R.S.; Hon. Mountstuart Elphinstone; Colonel Sir Augustus Frazer, R.A., F.R.S.; Captain Hall, R.N., F.R.S.; W. R. Hamilton, Esq., F.R.S.; R. W. Hay, Esq., F.R.S.; J. Cam Hobhouse, Esq., F.R.S.; Captain Horsburgh, F.R.S.; Colonel Jones, R.E.; Captain Mangles, R.N., F.R.S.; Thomas Murdoch, Esq., F.R S.; Right Hon. Sir George Murray, G.C.B., F.R.S.; Captain Lord Prudhoe, R.N., F.R.S.; Captain Smyth, R.N., F.R.S.; H. G. Ward, Esq.

The Chairman then addressed the following observations to the Meeting, explanatory of the general views of the Society:—

The Geographical Society of London being now established, the Provisional Committee cannot close its proceedings without adverting to the gratifying fact of there being enrolled, on the List of its Members, within so short a space of time, considerably more than four

four hundred names. From this great and increasing number, and still more from the general character of the subscribers, it is fair to conclude that a favourable opinion has been formed of the utility likely to result from the labours of such a Society. The *degree* of utility, however, which will be really effected, the Committee deem it almost unnecessary to observe, must depend on the attention and assiduity which the President, the Vice Presidents, and the Council may bestow on its concerns, quite as much as on the stock of knowledge they may bring to the consideration of the several subjects that will come before them. And not on the Council alone will depend the extent to which the useful labours of the Society are expected to be carried, but in a very great degree also on the assistance which they may receive from the many individuals eminent in the arts, sciences, and literature, and from the distinguished officers of the army and navy, whose names appear on the List of Members.

The many opportunities that are afforded to officers of the army, while on service abroad, and the promptitude and ability with which they avail themselves of them, (as the office of the Quarter-Master-General and the Board of Ordnance so amply testify,) are the best pledges of what may reasonably be expected from that quarter; and the more so since the Committee has had the satisfaction to witness the readiness with which so many distinguished officers of the Royal Artillery and Engineers have come forward to join the Society.

With the same confidence the Committee look for aid from the officers of the sister service, who on their own peculiar element in particular, will, it is hoped, be assisted by other experienced navigators, whether of and belonging to the Corporation of Trinity, the East India Company, or to any other maritime service. On the exactitude of the minutest details of hydrography must always depend the safety of commerce and navigation. Numerous dangers unquestionably exist in various parts of the ocean, that have not yet been ascertained; while others that have no existence still figure on our charts, to the dread of navigators. It has been well observed, that "the man who points out, in the midst of the wide ocean, a single rock unknown before, is a benefactor of the human race;" and scarcely less so is he, who, after careful examination, is able to decide that a rock or shoal, which appears on a chart, is either misplaced, or has no existence.—These, it is true, may not be ranked among brilliant discoveries; but the smallest obstruction, whether rock or shoal, that exists in the ocean, may have been, and, so long as its exact position remains unascertained, is still likely to be, the cause of destruction to life and property. It may also be noticed that many practical observations are still desirable on the prevailing winds and currents, and more particularly on tides, of which there are various peculiarities among the islands and along the different coasts of the ocean, concerning which facts and observations are still wanting, for establishing one general theory that shall be found applicable to every part of the globe.

Every

Every accession, therefore, to hydrographical knowledge,—a real danger discovered,—a fictitious one demolished,—or a peculiarity ascertained,—must be of great importance to navigation, and a fit object for promulgation by the Society.

The Committee, however, are also willing to hope, that many valuable contributions on geographical subjects will be received from other individuals, whether on the List of Members or not, than those who are thus professionally qualified and invited to furnish them; particularly from such of their countrymen as have permanent residences abroad, from the various public authorities in the British colonies, and from those who have travelled, or may yet travel, in foreign countries. It is not for the Committee to specify in detail the various points of information which should engage the attention of the traveller; but they may observe that every species of information, connected either with physical geography or statistics, if it have only accuracy to recommend it, will be acceptable; and in cases where the stock of information, generally, in the hands of any individual, is not of sufficient magnitude or importance to form a volume for publication, if sent to the Society, it will be made available, in some form or other, in its Transactions. The routes, for example, which travellers may have pursued through portions of countries hitherto but imperfectly known, or inaccurately described,—the objects of natural history that may have presented themselves,—the meteorological and magnetic phænomena that may have been observed,—the nature of the soil and its products, of its forests, rivers, plains, mountains, and other general features of its surface; but above all, the latitudes and longitudes of particular places which the resident or traveller may have had the means of determining to a degree of precision on which he may rely;—such notices of detached portions of the earth's surface, when regular surveys cannot be held, are of extreme importance, as furnishing the only means by which anything approaching to correctness in our general maps can be attained. And the Committee cannot, therefore, entertain a doubt, that it will constitute a part of the Transactions of the Society to publish such detached pieces of information bearing on such of these points, as may be thought of sufficient interest and importance to be communicated for the use of its Members, and of the public at large.

There are many other means besides those now mentioned by which geography may be advanced, but which are too numerous to be here specified at length. In addition to the few, however, which have herein been noticed, as well as in the printed prospectus already circulated, the following points may be briefly stated, as being among the most important that will probably engage the attention of the Society:—

1. The composition of maps, illustrative of particular branches of geographical knowledge, more especially those relating to orology, hydrology, and geology.

2. The establishment of new divisions of the earth's surface, formed upon philosophical principles, and adapted to different departments

partments of science; more especially as regards those divisions which are founded on physical and geological characters, on climate, and on distinctions of the human race, or of language.

3. A more uniform and systematic orthography than has hitherto been observed, in regard to the names of cities and other objects; and a more precise and copious vocabulary, than we at present possess, of such objects.

4. The preparation and improvement of road-books for different countries, of gazetteers, and of geographical and statistical tables, and all such matters as are of general utility.

The Committee cannot take upon itself to pronounce to which, of so many important considerations as have been enumerated, the attention of the Society should be *first* directed; the order of precedence must obviously, in some measure, depend on the means rather than the wishes of the Council; but the Committee are willing to hope that, sooner or later, most or all of the subjects mentioned will engage that attention of the Members to which they appear to be fairly entitled; and that the range of investigation will in no respect be less comprehensive than the title of the Society implies.

In making these observations, which have reference chiefly to facts, the Committee wish, however, to guard themselves against any supposition, that might be entertained, of their being hostile to theory; or of recommending to the Society to limit the reception of communications to such only as are the result of actual observation and experiment. On the contrary, they are fully aware that great benefits have been, and may yet be, derived from speculative geography. Theories that do not involve obvious absurdities or impossibilities, but are supported by reasonable probabilities, may serve as guides to conduct to important discoveries; by exciting curiosity they stimulate inquiry, and inquiry generally leads to truth. Reasonings and suggestions, therefore, in regard to parts of the world deserving of minuter investigation, which are little known, or of which no good account has yet been given, the routes to be observed in examining them, the chief subjects of inquiry, and best modes of overcoming the probable difficulties that may occur in the research,—all these will form proper subjects for admission into the proceedings of the Society.

And lastly, The Committee having reason to think, that at no great distance of time, the Society will be able to obtain suitable apartments for the reception of books, maps, charts, and instruments, they would venture to suggest, that donations of such materials as may tend to the elucidation and extension of geographical science would afford facilities to the attainment of its views. And they are willing to hope that, aided by such means, a library of books and manuscripts on geographical subjects, with a collection of charts and maps, may be formed, that will not be undeserving of public approbation and patronage.

XLVII. *In-*

XLVII. *Intelligence and Miscellaneous Articles.*

BROWN'S MICROSCOPICAL OBSERVATIONS ON THE PARTICLES OF BODIES.*

MUNCKE, of Heidelberg, finds the following a simple and easy mode of showing the motions of particles:—Triturate a piece of gamboge, the size of a pin's head, in a large drop of water on a glass plate; take as much of this solution as will hang on the head of a pin, dilute it again with a drop of water, and then bring under the microscope as much as amounts to half a millet-seed: there are then observable in the fluid small brownish yellow, generally round (but also of other forms) points, of the size of a small grain of gunpowder, in distances from one another of 0·25 to 1 line. These points are in perpetual slower or quicker motion, so that they move through an apparent space of 1 line, in from 0·5 to 2 or 4 seconds. If fine oil of almonds be employed in place of water, no motion of the particles takes place, while in spirit of wine it is so rapid as scarcely to be followed by the eye. This motion certainly bears some resemblance to that observed in infusory animals, but the latter show more of voluntary action. The idea of vitality is quite out of the question. On the contrary, the motions may be viewed as of a mechanical nature, caused by the unequal temperature of the strongly illuminated water, its evaporation, currents of air, and heated currents, &c. If the diameter of a drop is placed at 0·5 of a line, we obtain, by magnifying it 500 times, an apparent mass of water of more than a foot and a half the side, with small particles swimming in it; and if we consider their motions magnified to an equal degree, the phænomenon ceases to be wonderful, without, however, losing anything of its interest.—*Jameson's Journal*, July 1830.

VEGETABLE MILK OF THE HYA-HYA TREE OF DEMERARA.

Dr. Christison has lately analysed this substance. Its principal properties are the following: In the state in which it arrived in this country, it consisted of a small portion of a clear watery-like fluid, and a white concrete, cellulated substance, not unlike pressed curd, which filled nearly the whole bottle. It had an odour somewhat like that of cheese, with a slight peculiar aroma and scarcely any taste; the watery portion appears to contain a little acetic acid. The concrete matter is of snowy whiteness, brittle and pulverizable when cold, but easily softened by an increase of temperature. At 100° Fahr. it becomes ductile and viscid, and does not recover its original firmness and hardness for more than a day. At high temperatures it becomes more fluid, the vapour is combustible with much flame and smoke. Water cold or boiling has no action on this substance; heated alcohol acts slightly on it. Sulphuric æther dissolves it rapidly, leaving only four per cent. of a soft viscid mass.

From the preceding and other experiments, Dr. Christison concludes that the juice of the hya-hya tree consists of a small portion of

* See Phil. Mag. and Annals, N.S. vol. iv. p. 161. vol. vi. p. 161.

of caoutchouc and a large proportion of a substance possessing in some respects peculiar properties, which appear to place it intermediate between caoutchouc and the resins, to the latter of which it bears the greatest resemblance. Dr. Christison suspects that it is not nutritive, and he observes that it differs totally from the juice of the *Palo di Vaca* described by Humboldt as supplying the vegetable milk of the province of Caraccas in South America, as well as from the juice of the papaw tree; the solid contents of the former, from a late analysis by Boussingault and Mariano di Rivero, appear to be a large proportion of a substance analogous to fibrin, and a little wax and sugar, while from the experiments of Vauquelin the juice of the papaw tree contains two principles analogous to albumen and caseïn. —*Ibid.*

PRECIPITATE OF CHLORIDE OF IODINE BY SULPHURIC ACID.

Chloride of iodine (chloriodic acid) is, as is well known, very soluble in water; but it was not till lately observed that it is readily precipitated from solution by sulphuric acid: this occurs when a great excess of the acid is poured into the solution, and the chloride of iodine precipitates unchanged. M. Serullas, who made this experiment, also mixed muriatic and iodic acids in proper proportions; and he observed, that on pouring sulphuric acid into the watery solution of this mixture, the iodine and chlorine precipitated in the state of chloride of iodine, whilst the other elements of the acids combined to form water.

It may perhaps be thought, from this experiment, that the solution of chloride of iodine in water is a simple mixture of muriatic and iodic acid; but this opinion is inadmissible, because it is well known that by means of æther all the chloride of iodine may be separated from water.—*Journal de Pharmacie*, April 1830.

BUTYRIC ACID IN URINE. EXISTENCE OF LACTIC ACID.

M. Berzelius has informed M. Chevreul, that by treating urine with sulphuric acid he has succeeded in separating butyric acid from it: he has also stated that lactic acid, which some chemists were inclined to consider as acetic acid combined with a peculiar matter, is really a peculiar acid. One of the experiments on which this opinion is founded, is, that when ammonia is combined with lactic acid, and the product is distilled, no acetate of ammonia is obtained, which ought to happen if lactic acid were a combination of acetic acid.—*Ibid.*

FUMING NITRIC ACID.

In his laboratory, at the temperature of 14° Fahr., M. Mitscherlich heated several pounds of fuming nitric acid, slowly, in a retort, the neck of which and the receiver were cooled by a mixture of muriate of lime and snow; no gas was evolved, but the acid condensed in the receiver consisted of two layers, the separation of which took place after every mixture by agitation. The lighter fluid boiled at 82°·4 Fahr. retaining that temperature until the last portion had evaporated; its specific gravity was 1·445; it decomposed when mixed

with water into nitric acid and nitrous oxide, and had all the other properties of the compound of nitric and nitrous acid discovered by M. Dulong.

The heavier fluid being heated, its boiling point rose gradually from 82°·4 to above 259° Fahr. as the distillation proceeded.

This fluid is of an intense red colour like common fuming nitric acid; it becomes colourless when about half of it has been distilled. The product consists of equal quantities of a light and heavy fluid; the latter has a sp. gr. of 1·539. Common fuming nitric acid presents similar results.

It appears from these experiments that fuming nitric acid is a solution of hyponitric [hyponitrous ?] acid in nitric acid; the latter is capable of dissolving only about half its weight; so that when common fuming nitric acid is distilled, there are obtained a heavy fluid, which is a saturated solution of nitrous acid in nitric acid, and a lighter one, which is hyponitric [hyponitrous ?] acid.—*Ann. de Chim.* xliii. 220.

PREPARATION OF SUGAR FROM STARCH.

M. Heinrich says, that from one to two parts of sulphuric acid for each 100 parts of potato starch is sufficient, if the heat applied be a few degrees above 212° Fahr.: and also, that then two or three hours are sufficient to give crystallizable sugar. He applies the heat in wooden vessels by means of steam.—*Royal Institution Journal*, June 1830.

SULPHATE OF POTASH AND COPPER.

When equal quantities of sulphate of potash and sulphate of copper are mixed, a particularly bright green precipitate is gradually formed, which Vogel considered as a subsalt. Having been analysed by Brunner, it appears to consist of

Oxide of copper	39·23
Potash	12·12
Sulphuric acid	39·70
Water	8·94
	100·00

Ibid.

ACTION OF SULPHURIC ACID UPON ZINC, AND CAUSES PRODUCING ELECTRICITY.

M. Arago read a letter, dated May 19, to the French Academy, from M. Auguste Delarive of Geneva. This letter relates to the different subjects above named.

"I had," says the author, "been singularly struck with the enormous difference which exists between the action of diluted sulphuric acid upon the zinc of commerce, and the much less rapid solution which occurs when the zinc is purified by distillation. Having succeeded by means of a very simple apparatus in measuring the quantity of hydrogen gas evolved in a given time by the action of diluted acid upon zinc, I endeavoured to determine what were the circumstances which rendered the evolution more or less rapid. The temperature of the

the liquid, its degree of concentration, and the nature of the zinc, appeared to me to be the three most important circumstances; and after having studied them minutely, I arrived at the following results: 1st, That the proportion of water and sulphuric acid, which occasions the production of the greatest quantity of hydrogen gas by acting upon zinc, is that in which acid of sp. gr. 1·848 constitutes from 30 to 50 per cent. by weight of the solution. 2ndly, That this same proportion is that which, put into the voltaic circuit by means of a double galvanometer, is the best conductor of electricity. 3rdly, That the difference observed to exist between distilled zinc and that of commerce, appears to be owing to the foreign substances which are mixed with the latter, and particularly iron, which occurs in greater or smaller quantity. 4thly, That the influence of these heterogenous substances appears to have an electrical effect, resulting from their mixture with the more oxidable particles of the zinc."

"I have made many experiments with different mixtures of distilled zinc, and filings of iron, lead and other metals, and I have always found that the distilled zinc, into which I had thrown one or two per cent. of iron filings while it was melted, was of all, that which gave the strongest evolution of hydrogen with diluted acid; the zinc of commerce alone produced as much under the same circumstances, the chemical analysis of which showed that it contained a quantity of iron perfectly similar to that of the artificial mixture. That the influence of this iron was electrical, appears to be proved by many circumstances; such as the relation which exists between the electrical conductibility of the diluted acid and its action upon the zinc, the nature of the action which the diluted acid exerts upon every mixture of zinc and other metals; and lastly, even the manner in which the effect of these molecular currents may be excited, by inserting in the surface of the distilled zinc a great number of small platina points, instead of mixing it with other filings."

"It appears to me, therefore, that there occur on the surface of the zinc acted upon, a great number of molecular currents, which going from each particle of zinc to each heterogeneous particle occurring in the metal, traverse the acidulated water, and decompose it with the greater facility as its conducting power is good, and produce that increase of temperature which always results from the passage of electric currents through a liquid. The order of the electromotive powers of the different mixtures of zinc and heterogeneous metals, and the intensity of the currents to which they give rise, form a series of facts, serving to confirm the preceding explanation."

As to the causes which produce electricity, M. Delarive observes: "I have ascertained that contact alone, unconnected with any efficient cause, cannot by itself occasion electricity, either in the form of *currents* or in that of *tension*. Independently of the processes which I have already described, I have employed others, such as using condensers of different kinds, and condensers placed in different media; and if by this last process I have arrived at results which differ from those of M. Pfaff, it is, as I have ascertained, because the smallest quantity of moisture remaining in the air or in a gas, occasions a chemical action

 upon

upon the zinc surface of the condenser, and consequently produces (whatever M. Pfaff may say) an electrical effect, the nature of which always corresponds with what ought to occur, according to the chemical theory. I have not confined myself to negative experiments, although their number and agreement gave me the utmost confidence in them; but I have endeavoured also to discover such as would yield positive results. In this way I have obtained signs of electricity, under circumstances in which according to the theory of contact I ought not to have procured the slightest trace. I will give an example of this: In each end of a cylinder of wood from ten to twelve centimetres long, and from one to two in diameter, I inserted a plate of zinc terminated externally by a brass end, which was soldered to it; holding in my hand the brass end of one of these plates, I touched the condenser (also of brass) with the brass end of the other. According to the theory of contact, I ought not to have obtained any sign of electricity, the two plates of zinc with brass being opposed and united by an insulated piece of wood, serving as a conductor from one to the other. Nevertheless, one of the ends of the wooden cylinder being a little more damp than the other, I obtained signs of electricity, the nature of which always agreed with the slight chemical action arising from the contact of the well cleaned zinc with the damp wood. These signs were positive, if I held the brass end of the plate inserted in the least damp end of the wood. In order to succeed in the experiment, the wood must be slightly moistened; the humidity which it acquires in moist air is quite sufficient; care must be taken to keep one of the ends drier than the other. It appears to me impossible, as I have endeavoured to show in my memoir, to reconcile this fact, when carefully examined in all its details, with the theory of contact."

M. Delarive, while he denies that the contact of two heterogeneous substances can be the cause of developing electricity, admits that it may often be a necessary condition. As to the real cause, it is always either

Physical, as heat. M. Becquerel has given, he observes, a complete and satisfactory analysis of this cause in his last memoir upon thermo-electrical currents;—or

Chemical. M. Delarive has already shown in his preceding memoirs, how he regards this kind of action;—or

Mechanical. There still remains, the author observes, much to be done, in arranging according to general principles the mechanical processes for exciting electricity, such as friction and pressure. He has already had occasion to make a considerable number of observations relating to friction. When rubbing, he says, a very dry finger, a cork, or a piece of wood, upon a piece of metal, of a cubic form, for example, placed upon a condenser, electrical appearances of astonishing intensity are developed, which are sometimes negative and sometimes positive. The nature of the electricity depends upon the kind of metal, its figure, temperature, the manner in which it is rubbed, whether upon an edge or a face; the nature of the rubbing body, which should always be an imperfect conductor, has but little influence. M. Delarive states, that in all his experiments he carefully avoided

avoided placing the metal in immediate contact with the condenser, putting a very thin plate of ivory between them.

M. Delarive has also announced an important fact. He has ascertained that the transmission of electricity from one conductor to another varies sensibly, according to the direction of the current; that is to say, for example, that positive electricity passes more readily from copper to zinc, than from zinc to copper. The discovery of this fact serves to explain many phænomena hitherto regarded as anomalous. The author states that he was led to this discovery by the observations of M. Fourier relative to the passage of heat through substances according to the order in which they are arranged.—*Le Globe*, June 30, 1830.

ATMOSPHERIC CARBONIC ACID.

M. Théod. De Saussure has made numerous experiments to determine the variations of carbonic acid in the atmosphere, and published them in the memoirs of the Physical and Natural History Society of Geneva. The following results are extracted from the *Bibliothèque Universelle* for June 1830.

The carbonic acid was absorbed by barytes water, and the carbonate precipitated was estimated to contain 22 per cent. of carbonic acid. The experiments were made at Chambeisy, about three quarters of a league distant from Geneva, and 16 metres above the level of the lake; 10,000 volumes of air contain 4·15 of carbonic acid, as the mean of 104 experiments made day and night and at all seasons of the year; the air was taken four feet above the ground; the greatest quantity of carbonic acid was 5·74, and the smallest 3·15.

An increased quantity of rain appears to diminish that of the carbonic acid, either by dissolving it or in causing the soil to do so; a litre of fresh rain-water, which did not render lime-water turbid, gave in summer, by an hour's boiling, 20·5 cubic centimetres of air, which contained 13·46 of azote, 6·73 of oxygen, and 0·31 of carbonic acid: and it appears from a very elaborate set of experiments, that the quantity of carbonic acid is generally larger as that of the rain is smaller; —thus in June 1827, the quantity of rain was 9 millimetres, and that of the carbonic acid 5·18, in 10,000 of air; while in September 1829, the rain was 254 millimetres, and the carbonic acid only 3·57 volumes in 10,000 of air.

It was found that during the night the quantity of carbonic acid was greater than that of the day in the proportion of 4·32 to 3·98; but if the wind be strong, then scarcely any difference occurs. The greater quantity of carbonic acid occurring in the night is attributed to the want of decomposition which occurs by vegetation during the day.

A short frost and which does not penetrate the earth to more than an inch, does not appear to cause any variation in the quantity of carbonic acid; but when the frost continues long, the dryness which it occasions increases the proportion. In the beginning of January 1829, the ground was lightly covered with snow, and the quantity of carbonic acid increased to 4·57; towards the end of the month it thawed for several

several days, and the acid was then reduced to 4·27. At the beginning of February the frost recommenced, and in the middle of the month the ground was frozen to the depth of eight inches; the carbonic acid rose to 4·52, it then thawed and the acid was reduced to 3·66.

The mean result of numerous experiments showed that the quantity of carbonic acid in 10,000 of air was less when taken over the lake of Geneva than at Chambeisy in the proportion of 4·39 to 4·60.

It was also found that the variations in the quantity of carbonic acid dependent upon season and night and day, occurred also in the air taken over the lake.

On comparing the quantity of carbonic acid found in the air of Geneva with that of Chambeisy, the former was found to be greater in the proportion of 4·68 to 4·37; these were the mean results of many experiments. It was also found that the quantity of carbonic acid is greater during day in the city than in the country, that the variations occasioned by the seasons are analogous, and that the quantity of carbonic acid increases more by the influence of night in the country than in the town.

On comparing the quantities of carbonic acid found in the air of the plains with that of the mountains, it was observed that the latter contained the most; this difference is explained by the consideration that the decomposition of the acid occurs principally where vegetation is most abundant, as it is in the plains, and that the gas is absorbed by the earth there, because it contains more rain-water. It appears also that the night has but little influence in increasing the carbonic acid of the mountain air.

ARSENIURETS OF HYDROGEN.

M. Soubeiran concludes from his experiments on these compounds, that in the present state of science only two arseniurets of hydrogen are known; one is solid, and is composed of

Arsenic	97·416
Hydrogen	2·584
	100·

These proportions are considered by M. Soubeiran as equivalent to 1 atom arsenic + 2 atoms hydrogen; but if the atom of arsenic be 38 and that of hydrogen 1, then it will be constituted of one atom of each; this is the hydruret of arsenic. The other compound is gaseous, and is composed, according to M. Soubeiran, of 1 atom of arsenic + 3 atoms hydrogen; or

Arsenic............	96·18
Hydrogen..........	3·82
	100·

If however the atom of arsenic be reckoned 38, then arseniuretted hydrogen gas consists of 2 atoms arsenic 76, and 3 atoms hydrogen 3.

This gas is always the same by whatever process it may have been procured, except an admixture of hydrogen. It is best prepared and with the greatest certainty by acting with acids upon arseniuret of

of zinc prepared by fusion. The alkaline oxides, especially in the state of hydrate, are converted by arsenic into hydrogen, metallic arseniuret, and into arseniate or arsenite. The deposit formed by the slow action of the air, or that of chlorine on arseniuretted hydrogen, is not hydruret of arsenic as has been supposed, but metallic arsenic; when the arseniurets of tin and zinc are treated with acids, no hydruret of arsenic is formed, but they leave a residue of super-arseniuret, unattackable by acid.—*Journal de Pharmacie*, June 1830.

PREPARATION OF BICARBONATE OF SODA.

M. Creuzberg has found a ready mode for the manufacture of this salt, in the circumstance that the dry alkalies absorb carbonic acid much more quickly than when in solution. Carbonate of soda is therefore deprived of much of its water by efflorescence, and is then subjected to a current of carbonic acid gas until the bicarbonate is formed; the time when this takes place is rendered evident by the evolution of heat, and the exhalation of water, which is deposited in drops upon the interior of the vessel.—*Bibl. Univ., Roy. Inst. Journ.*

ANALYSIS OF MUSTARD-SEED. BY M. PELOUZE.

Beaumé, and after him MM. Deyeux and Thiberge, has stated the existence of sulphur in the essential oil of mustard. MM. Henry jun. and Garot found among other principles a peculiar acid, which they called *sulpho-sinapic acid.*

After showing that the substance upon which these chemists operated, could not be pure on account of some atomic discordances in the compounds it is stated to have formed with various bases, M. Pelouze maintains that the acid is merely the hydrosulphocyanic existing in the state of sulphocyanuret of calcium: it appears, however, that the sulphur which the seed contains does not exist entirely in this state, but also uncombined; for when the seed is boiled with potash, acetate of lead shows the presence of sulphuret of potassium.

Hydrosulphocyanic acid (or rather sulphocyanic acid) may be obtained from the seed by the direct action of dilute sulphuric acid upon strong decoctions of it, but the quantity is small. The following is given by M. Pelouze, as the composition of mustard seed:

Volatile oil.	Yellow colouring matter.
Fixed oil.	Albumen.

Crystallizeable white colouring matter.—Discovered by MM. Henry and Garot.

Bimalate of lime.	Sulphocyanuret of calcium.
Citrate of lime.	Uncombined sulphur.

Ann. de Chimie, June 1830.

ON SALICINE. BY MM. PELOUZE AND JULES GAY-LUSSAC.

Salicine, a peculiar vegetable compound obtained from the bark of the willow, is a perfectly white body which crystallizes in acicular prisms. Its taste is very bitter, and partakes of the aroma of the bark itself. One hundred parts of water at 68° Fahr. dissolve 5·6 parts. In hot

hot water its solubility is much greater, and in boiling water it appears to dissolve in any quantity. It is also soluble in alcohol; but æther and the essential oils do not dissolve it, except perhaps the oil of turpentine.

Sulphuric acid poured upon salicine, assumes a very fine red colour, perfectly resembling that of bichromate of potash; with nitric and muriatic acids it forms colourless solutions. It is not precipitated from its solution in water by tincture of galls, by gelatine, acetate or subacetate of lead, alum, or tartarized antimony.

When boiled with excess of lime water, it does not saturate it. Oxide of lead is not dissolved by it. At a few degrees above the temperature of boiling water it melts, and on cooling becomes a crystalline mass; it loses no water during this operation. If the heat be much greater than that of its melting point, it becomes of a lemon yellow colour, and as brittle as a resin.

Salicine, burnt with oxide of copper in a proper apparatus, yielded a gas entirely absorbable by potash. The mean of two careful analyses gave, as the composition of salicine,

Carbon	55·491
Hydrogen	8·184
Oxygen..........	36·325
	100·

or in proportions,

Carbon	2·028	proportions
Hydrogen	2·004	
Oxygen	1·000	

Salicine is therefore formed of

Carbon	2·000	proportions
Hydrogen	2·000	
Oxygen	1·000	

Its composition may perhaps be represented by two volumes of olefiant gas and one volume of oxygen.—*Ibid.*

SULPHATE OF BARYTES FORMING A VEIN IN CANNEL COAL.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

I was a few weeks since presented with a mineral composed of sulphate of barytes and carbonate of lime, found in a cannel pit, on the estate of the late Duke of Bridgewater, known by the name of Water-gate Pit; which is situated in the township of Middle Hulton, about two miles south-west of Bolton, Lancashire.

This mineral is found running through the cannel in the form of a vein, about an inch thick: it is crystallized sulphate of barytes, irregularly mixed with transparent crystals of carbonate of lime; to the outides a quantity of iron pyrites is attached; its specific gravity varies: some fragments, when freed from pyrites, are of the specific gravity 4·63, others are as low as 4·19.

I have

I have carefully analysed the mineral (when freed from pyrites), and find its composition to be as under:

Specific gravity.	Sul. Barytes per cent.	Carb. Lime per cent.
4·19	88	12
4·23	91·25	8·75
4·365	97	3
4·63	100	

As I am not aware of any notice of barytes having been found in the coal-measures before, it perhaps may present some novelty to your readers: otherwise I shall feel obliged by some of your correspondents, better acquainted with the subject than myself, communicating through the medium of your valuable journal any information connected with the geological relations of this mineral.

I am, Gentlemen, yours respectfully,

HENRY HOUGH WATSON.

Little Bolton, Lancashire, Sept. 10, 1830.

LAW OF PATENT INVENTIONS.

Extracts from the Evidence taken before the Select Committee of the House of Commons, on the Law relative to Patents for Inventions, 1829; *being the Statement made by Mr. Farey respecting Mr. Woolf's and Mr. Watt's Patents for Steam-engines; with Additional Facts in Support of that Statement.*

[Continued from vol. vii. p. 157.]

(P. 32 of the Report.) Do you consider that the term of fourteen years is sufficient in all cases?—By no means. In the case of Mr. Woolf's invention of working steam-engines by high-pressure steam acting expansively, (either in one or in two cylinders,) there was no profitable exercise of that invention for at least ten years out of the fourteen; and there was so much loss incurred at the first, that the profit made during the last four years did not repay the loss during the first period. (P. 33.) The extension since given to that invention is so important, that the existence of deep mining in Cornwall at this moment depends upon it. The difference in cost between the quantity of coals consumed by the engines now in use (which are all on Mr. Woolf's system), and by an equal force of engines such as were in use before he went into Cornwall in 1813, would absorb the profit of all the deep mining that is now carried on in Cornwall. I think Mr. Woolf is more entitled to a public reward for the services he has rendered, without any recompence, than any inventor who has ever been rewarded by Parliament.

(P. 136.) It would be a very good measure to reserve a portion of the revenue derived from the granting of patents, to accumulate and form a fund for the purchase of valuable secret inventions, like Mrs.

 Knight's,

Knight's, which are not likely to be disclosed by the inducement of any patent law however complete; and also to reward individuals like Mr. Woolf, whose inventions have not come into use during the terms of their patents, but have afterwards become of national importance.

Would you not in the latter case rather recommend an extension of the term of the patent?—Not in all cases. If the inventor has brought his invention to such perfection, that others, by merely copying what he has done, can practise it as well as himself, it would be best to throw it open. Mr. Woolf's was such a case; the engines made on his system by others, since the expiration of his patent, have performed as well as those made in the same interval by himself, and have even obtained a preference in some places: hence the public would probably have gained nothing by confining it longer in his hands; but now that he is seen to be left quite unrewarded for his long exertions, the circumstance, added to others of a similar nature, is very discouraging to men capable of making similar improvements; and I am of opinion that the public would gain by giving him a handsome reward. In other cases where there are not many persons capable of taking up the new subject, its progress will be greatly promoted by continuing the patent, because that compels the patentee to continue his exertions to extend the practice. That was Mr. Watt's case. If he had abandoned his engine to the public at the time his first patent would have expired, there was then no other person competent to go on with it, and give it that additional perfection which he attained during the prolongation of the term.

(P. 140.) Do you believe that many useful inventions would never have been prosecuted to the public advantage, if they had not originally been worked under a monopoly?—Mr. Watt's steam-engine may be quoted as a great example. At the time Mr. Watt made his invention in his own mind, in 1765, he was not a maker of steam-engines; and none of the makers of that day had sagacity enough to see the value of his discovery before he had made an engine; nor would any of them have prosecuted his plan before it was proved, even if he had made them a present of the invention, much more to give him any thing for it: hence he had no means of making any profit from his invention, or any prospect of repayment for the great expense and labour necessary to bring it to bear in practice, unless he could have secured it to himself for a long term. (P. 141.) The history of Mr. Woolf's invention is very similar, with the difference that Mr. Watt having through Mr. Boulton obtained an extension by Act of Parliament, he acquired a large fortune during the prolongation. Whereas Mr. Woolf's patent expired before the actual outlay had been repayed; so that he is left a real loser by his invention. The previous inventors of steam-engines, Mr. Savery in 1698, and Mr. Newcomen in 1710, were similar cases;—they lost money.

(P. 174.) Mr. Watt's invention, and the perfection he gave to it during the operation of this Act of Parliament, has proved of more value to the nation than can be calculated; probably as much as the inventions

inventions of Lord Dudley for smelting iron by pit-coal in 1619, or as those of Hargrave, Arkwright and Crumpton, for spinning machinery, about the same date as Mr. Watt. Dudley and Hargrave were not encouraged, but were persecuted, and their works destroyed by mobs: after Dudley's death his process lay dormant during a century, probably for want of support to him. These great inventions have had a close connection, and each one has promoted the progress of the other very greatly.

(P. 191.) Mr. Watt's is the most striking case amongst that very few where the inventor has been protected in his patent rights, for an adequate length of time, to enable him to perfectly establish his invention, and consequently recompense himself from the use of it. The great perfection which Mr. Watt attained, and the very general use into which he brought his steam-engines, for a great variety of applications, was entirely owing to that protection; and it is certain that the public would not have been benefited anything like so much, if his patent had not been prolonged by Parliament. Messrs. Boulton and Watt realized large fortunes by the patent. In addition to their profits as engine-makers, they took one-third of the annual savings in fuel made by their engines, compared with Newcomen's atmospheric engines performing the same work; that produced them a great revenue from Cornwall, where coals are dear, and the engines for draining mines very large and numerous.

The steam-engine is an invention from which the nation has derived immense wealth during the last century, and increasing means of wealth for the future. After the enunciation of the principle of action had been made by De Caus in 1615 and by Papin in 1690, the real inventors of the engine have been, Savery in 1698, Newcomen in 1712, Watt in 1769, Trevethick in 1802, Woolf in 1804, and Fulton, in America, in 1807. Of these Mr. Watt is the only one amongst us who has derived any adequate advantage or recompense for his labours. Mr. Woolf's failure of a recompense was entirely owing to the want of protection by an extension of his term; for his engines came into very general use in Cornwall soon after the expiration of his patent, in place of Mr. Watt's engines; and with such great advantage in economizing fuel, that Mr. Woolf would hav been amply recompensed if his term had been made as long as Mr. Watt's was.

Additional Facts relative to Mr. Woolf's Invention.

The specification to Mr. Woolf's patent of 1804 * claims the improvement of working steam-engines by high-pressure steam, acting expansively, either in one or in two cylinders, and describes the structure and mode of operation of engines of both kinds. It was at

* Printed in the Philosophical Magazine, vol. xxiii. p. 335. The system of working expansively with steam of the ordinary atmospheric elasticity was invented by Mr. Watt, who had a patent for it in 1782; he proposed to do it in one cylinder, and executed it with success. Mr. Hornblower proposed to do the same in two cylinders, and had a patent in 1784; but that plan did not in practice prove so advantageous as Mr. Watt's with one cylinder, and never came into use.

 that

that time quite a new proposal, but the general opinion of engineers was very unfavourable to it.

In the course of four or five years, Mr. Woolf made a few small rotative engines in London, some with two cylinders, others with one, but he met with no encouragement, and lost much money; until 1811, when he had brought his small rotative engines, with two cylinders, to such perfection, that on a well attested trial of a nine-horse engine and corn-mill, it ground 17¼ bushels of wheat by the consumption of one bushel of coals; and on a repetition of the trial, nearly 20½ bushels. Mr. Watt's rotative engines of that power will not do half so much.

From that time there has been a demand for Mr. Woolf's rotative engines with two cylinders, and a great number have been made; they have answered very well, and on an average consume only half as much fuel as the average of Mr. Watt's engines, exerting the same power. About 1813 Mr. Woolf obtained encouragement in Cornwall, and went to reside there: his partner Mr. Edwards continued the business by himself in London. In 1815 Mr. Edwards took out a patent in France, and sent over some engines, which were so much approved that he was induced to remove to Paris, where he has since made a great number of Mr. Woolf's engines. Also about the time that Mr. Edwards went to France, Mr. Hall began making Mr. Woolf's rotative engines with two cylinders, at Dartford, and has executed a great many excellent engines; most of them have been sent to France, where Mr. Woolf's engines are very common, and are greatly preferred to any others. Mr. Hall continues the business on an extensive scale.

Mr. Woolf's first engines for pumping water from mines were set up by him in 1814 at Wheal Abraham and at Wheal Vor mines in Cornwall; they had each two cylinders: their performance far exceeded that of any steam-engines ever made before. In the latter half of 1815, the two engines raised on an average 48 million pounds of water one foot high, for every bushel of coals they consumed. Thus:

Engines.	Half of 1815.	1816.	1817.	1818.
Wheal Abraham*	48·63	49·71	44·07	36·91
Wheal Vor ...	47·63	44·23	36·15	29·33
Average......	48·13	46·97	40·11	33·12

* Wheal Abraham engine raised 56·92 millions, on the average of all the month of May 1816. In 1818, this engine was put in order, and exact trials of its performance were made by Mr. Farey, with the steam kept up to a greater elasticity than usual, and acting with a greater extent of expansion than usual; it then raised 65·22 millions, on the average of two trials of eight hours and six hours each: that was the greatest effect ever produced by steam, until November 1827, when Woolf's engine at the Consolidated mines raised 67·10 millions, on the average of the month's working.

The steam cases for the cylinders of these engines were exposed to the open

To estimate the improvement thus effected by Mr. Woolf, by himself alone, without aid from any other engineers, and whilst he was opposed by many, the performance of the engines previously used in Cornwall must be examined.

Mr. Watt first introduced his engines into Cornwall in 1778, in place of Newcomen's atmospheric engines, which had been introduced there about fifty years before, but had never raised more than eight or nine millions. Mr. Watt did twice as much with his first engines, and three times as much, after he had applied his method of working expansively, for which he had a patent in 1782. It consists in stopping the supply of steam from the boiler to the cylinder, when the piston has only moved through a portion of its course, leaving it to be impelled through the remainder, by the expansive action of the steam already admitted into the cylinder, without any further expenditure of steam from the boiler.

Mr. Watt proposed in 1782 to work his engines by stopping the supply of steam when the piston had only moved one-fourth of its course, leaving three-fourths of the course to be performed by the expansive action; and although the force exerted by the steam during that expansion must continually decrease, nevertheless $2\frac{1}{4}$ times more power would be exerted on the whole than would be exerted by the same steam, if it acted without expansion. But it was found that such an extent of expansive action could not be realized in practice, because the steam used by Mr. Watt, when expanded to fill a quadruple space, becomes too feeble to impel a piston with effect.

Mr. Watt never retained the steam in his boilers much above the pressure of the atmosphere; they were always supplied with water through upright pipes, open at top to the atmosphere, and the lower ends immersed beneath the surface of the water in the boilers; the open ends of the feeding pipes being only eight feet high above that surface, the steam could not by any chance be retained in the boiler beyond $3\frac{1}{6}$ pounds per square inch more elastic than the atmospheric air: that was Mr. Watt's practice, and his successors in business at Soho still continue it. The term *low-pressure steam* can only with propriety be applied to the steam produced by such boilers.

Mr. Watt's engines with such boilers cannot be made to exert a competent power to drain deep mines, unless the supply of steam to the cylinder is continued until the piston has run through more than half its course; and on an average in practice, low-pressure steam is only expanded so much as to fill one and a half time the space it occupies in the cylinder, at the moment when the supply was cut off: even with that moderate extent of expansive action, the steam will exert nearly one half more power than would be exerted by the same steam acting without expansion, as it must do if the supply from the boiler to the cylinder were continued until the piston terminated its course.

open air without any clothing. See a description of the cylinders, Philosophical Magazine, vol. xlvi. pp. 116, 236, 319, & 398; and the performance for each month is stated in the Numbers of the succeeding volumes up to the end of 1818.

Mr. Watt's

Mr. Watt's engines, well constructed and well managed, will raise twenty-five millions for an average. They are still the only engines used in London for water-works, and in many districts for draining mines; they were the only engines used in Cornwall in 1814, when Mr. Woolf set up his first engines there. Under favourable circumstances, such as having little work to do, keeping up the steam as strong as can be retained in the boiler without overflowing the feeding pipes of eight feet high, and working with the utmost extent of expansive action that those circumstances will allow, using good coals, clean water, plenty of cold condensing water, and the boiler, cylinder and steam-pipes being properly clothed,—a well constructed engine of Mr. Watt's may raise thirty-two millions; but that is the utmost, and cannot be maintained in regular working. On the other hand a number of engines working incessantly at deep mines, under ordinary management and average circumstances, will not reach twenty millions, as is shown by the following account of the average performance of the engines in Cornwall in 1813 and 1814, when they were all worked on Mr. Watt's system of low-pressure steam acting expansively in one cylinder.

Date of Year.	Aggr. of the Engines in Cornw.				Aver. per Engine.		Annual Performance of the best Engines. Millions.
	No. of Engines	Aver. Mills.	Bushels per Ann.	Horse Power	Bushels per Month	Horse Power	
1813	24	19·38	770076	861	2672	35·9	26·65
1814	29	20·37	1002563	1176	2880	40·5	31·99
1828	56	37·33	1165866	2508	1735	44·8	77·29
1829	53	41·22	985435	2342	1550	44·2	76·23

In 1813 the highest performance attained by the best engines on Mr. Watt's system did not reach thirty millions, and the performance of all the engines averaged less than twenty millions; but in 1829, when all the engines were worked on Mr. Woolf's system of high-pressure steam acting expansively in one cylinder, the best of those engines averaged seventy-six millions, and the whole number (being more than twice as many as in 1813) averaged forty-one millions.

The great cause of the superiority may be thus explained: Mr. Woolf's engines are worked by high-pressure steam, which is retained in the boiler to an elasticity of between twenty-five and forty-five pounds per square inch more than the atmospheric air. Steam of that elasticity may be expanded to fill between five and eight times the space it occupied when first admitted from the boiler into the cylinder; and yet it will have a sufficient force to impel the piston with as much effect, during all that great extent of expansive action, as can be done in Mr. Watt's engines when the expansion is only from one space into two: hence Mr. Woolf's system renders the expansive action available to a greater extent than can be done in Mr.

Watt's

Watt's system. There are other concurrent causes for the great superiority, but that is the principal cause.

In his specification of 1782, Mr. Watt proposed to use steam equal to the atmosphere, with a far greater extent of expansive action than he or his successors have been able to realize in practice; and so Mr. Woolf in his specification of 1804 proposed to carry the expansion of high-pressure steam to a far greater extent than has ever been executed by himself or others *.

When Mr. Woolf began, it was a common notion that mere variation in the elasticity of the steam employed could no way affect Mr. Watt's invention of expansive working, and that the use of high-pressure steam, as proposed by Mr. Woolf, could never be advantageous. Such notions continued current, until he proved their fallacy by the great performance of his engines: but he did not attain that proof without exertion; for the difficulties of carrying his invention into effect were very great, and cost him some years of uninterrupted labour and great expense to overcome them. He encountered much active opposition from those who had made up their minds on his first proposals; and their unfavourable opinions prevented him from getting orders for any large engines until 1813.

The value of the improvement which Mr. Woolf made in the performance of engines, before his system was adopted or countenanced by other engineers, may be stated as follows. In 1814 the average performance of the twenty-nine engines that were reported, was 20·37 millions, and their consumption of coals was 100,2563 bushels. The price of coals being at that time $14\frac{1}{2}d.$ per bushel, the cost of coals was £60,570 per annum, or £2088 per annum for each engine, on an average.

The average performance of Mr. Woolf's engines at Wheal Abraham and Wheal Vor, during the three years and a half above cited, was 42·08 millions, or more than double the average of all Mr. Watt's engines in 1814: hence if all those engines had been replaced by engines such as Mr. Woolf had then made, more than half the expense of coals would have been avoided, being a saving of £29,300 per annum to the mine adventurers; or at the rate of full £1000 per annum saved in working each engine, on an average of the whole number, each engine exerting about forty-horse power.

The expense and complication of Mr. Woolf's engines with two cylinders being found objectionable, he altered an old Watt's engine at Wheal Abraham in 1816, to work by high-pressure steam, with an increased extent of expansive action, in one cylinder; and the improvement in its performance proved that two cylinders are not

* If it is represented as a failure, that Mr. Woolf did not accomplish all that he anticipated in 1804, still that is no reason for overlooking what he did accomplish by himself in 1811 and 1815, or how much he then surpassed all that had been done before: nor can any such imputation of failure justify the omission of his name by those writers who undertake to state the extension that has been given to Mr. Watt's discovery of expansive working, by using high-pressure steam; for that extension is not due to Mr. Watt in any part, but to Mr. Woolf entirely.

essential

essential to the successful practice of his system*. He altered another old engine at Wheal Unity, by adding a small cylinder to it; the performance was improved in about the same degree as that of the old engine with one cylinder.

In 1816 an entire new engine was made at Dolcoath mine, by Messrs. Jeffery and Gribble, to work with high-pressure steam acting expansively on Mr. Woolf's system, in one cylinder of seventy-six inches diameter; it answered extremely well, although it never did so much as the engines with two cylinders had done, whilst they were new and in good order: but as they materially fell off in 1817 and 1818, when they got out of order, and as the Dolcoath engine kept up to a steady performance of about forty millions, the use of one cylinder for Mr. Woolf's system obtained the preference; and in a short time after Mr. Woolf's patent expired, most of the old Boulton and Watt's engines in Cornwall were altered to work by high-pressure steam on his system: some few had an extra cylinder added, but commonly the old cylinder was retained. The advantage of the change from low-pressure to high-pressure steam, on Mr. Woolf's system, was manifest in all cases; but it was greater or less, according as the steam was used stronger, and with more or less expansive action.

All the new engines since erected in Cornwall have been made expressly to work on Woolf's system, and always with one cylinder, excepting one instance of two cylinders. In 1820 Mr. Woolf made two engines for the Consolidated mines, each with one cylinder ninety inches diameter; but as neither of those, nor the Dolcoath engine, ever did so much as the engines with two cylinders had done at first, Mr. Woolf still felt inclined to prefer his original plan. Accordingly in 1824, having undertaken to make two large engines at Wheal Alfred, he prevailed on the adventurers to go to the expense of making one of them with two cylinders, of forty and seventy inches diameter, the other engine being the same as those at Consolidated mines, with one cylinder ninety inches diameter; both engines were worked with high-pressure steam. The performance in 1825 averaged 40·01 millions with two cylinders, and 42·15 with one cylinder: this was considered decisive against two cylinders†; and no engines have since been made in Cornwall either by Mr. Woolf or others, except with one cylinder, to work on his system.

The performance of those engines was very slowly and gradually increased, as appears by the following annual averages of the highest performances that are to be found amongst them each month. Until 1826 their performance remained below that of the first engines with two cylinders in 1816, which then averaged 46·97 millions. Previous

* The same fact had been ascertained years before, in respect to Mr. Watt's system of working expansively by low-pressure steam; for Mr. Hornblower, who practised that system in two cylinders, did not succeed so well as Mr. Watt himself, who only used one cylinder.

† The engine with two cylinders had boilers on a complicated plan which did not answer well, and the other engine had very good boilers. If both engines had been worked with equally good boilers, the two cylinders would have made the best performance.

to 1826 the steam cases of the cylinders were not clothed, but exposed to the air.

Years.	Millions.	Years.	Millions.
1816	36.3	1823	42·1
1817	41·6	1824	43·5
1818	39·3	1825	45·4
1819	40·0	1826	45·2
1820	41·3	1827	59·7
1821	42·8	1828	77·3
1822	42·5	1829	76·2

The advance made in 1827, and since that time, has been effected by good management of the engines, chiefly by clothing all the steam vessels, and thus preventing any needless waste of heat by radiation, also by using better boilers*; but the engines are strictly according to Mr. Woolf's system of high-pressure steam acting expansively in one cylinder.

Great credit is due to Captain Samuel Grose, who began the race of improvement in management; first in an engine which he made at Wheal Hope in 1825, and still more in another which he made the next year at Wheal Towan, with an eighty-inch cylinder; in 1827 it averaged 58·18 millions, its highest being 62·22 in July.

At the end of 1827 Mr. Woolf removed the ninety-inch engine before mentioned at Wheal Alfred (that mine having ceased working) to the Consolidated mines, and by good management and clothing it raised 64·42 millions on the average of the last three months, the highest being 67·10 in November.

In 1828 Captain Grose brought the annual average performance of Wheal Towan engine to 77·29, the highest being 87·05 millions in April †. Mr. Woolf's engine averaged 62·57, and its highest was 67·56 millions in May. These striking examples stimulated the exertions of all the Cornish engineers to take the same care in management, and with such success that the average of all the engines in 1829 was 41·22 millions; although in 1814, before Mr. Woolf's system was begun, it was only 20·37, or less than half. The number of engines and the power exerted by them is more than doubled, whilst the quantity of coals consumed by them is sensibly lessened.

The importance of such an increase of power from the same fuel, to the success of mining in Cornwall, may be estimated by the following account of the Consolidated and United mines, which are worked by one company of adventurers, and form the largest mining establishment in existence.

The United mines are worked to a loss, and are only kept drained

* See Mr. John Taylor's paper on these boilers, Phil. Mag. and Annals, N.S. vol. i. p. 126; they are long cylinders containing cylindrical tubes within them, for the furnaces: on a plan which was first brought into use for high-pressure steam by Mr. Trevethick in 1804.

† See Mr. John Taylor's account of the performance of this engine in May 1830, when on an accurate trial of about two hours and a half working, it raised 92·33 millions.

to about one-third of their depth, in order to cut off some water which would otherwise flow into the Consolidated mines. These mines have been more productive during the last seven years than the average of mines in Cornwall. The following particulars are collected from the accounts which have been printed annually in a series of reports by Mr. John Taylor, and from the monthly reports on the engines.

The Consolidated mines recommenced working in 1819, after lying drowned for fourteen years, and £65,000 was advanced by the new adventurers to bring them into operation. During the years 1819, 1820 and 1821, the expenditure exceeded the returns by £74,078; but during the years 1822, 1823 and 1824, a profit of £51,561 was made.

At the end of 1824, £10,000 more was subscribed to continue the United mines, which were given up by their original proprietors. The capital to be repaid to the adventurers at the beginning of 1825, including interest then due upon the several advances, was £55,382. During the last five years, the Consolidated and United mines together have produced a profit of £63,604, whereby all the capital subscribed, together with interest upon it, has been paid off, and an actual gain was made in 1829, in addition to the value of the stock of materials on the mines.

The total expenditure in all the eleven years, has been £824,585, and the returns £865,672: hence the profit beyond the repayment of the capital subscribed has been £41,087 in eleven years; or interest at five per cent. per annum being allowed on the sums subscribed until the periods of repayment, the clear gain is stated in the printed accounts to be only £10,244 to the adventurers *.

The expense of draining the water from both mines, as stated in the annual accounts of the last five years, has been decreasing each year from £17,776 to £11,958 per annum; although the monthly reports on the engines show that the number of engines has been increased from four to eight, and the power exerted by them increased from 432 to 513 horse power. The cost of drainage has averaged £13,826 per annum. The average performance of the engines has been improved from 31·04 to 51·81 millions, during the last five years, averaging 39·36 millions, being more than double 19·38 millions, which was the average performance of Mr. Watt's engines in 1813, when Mr. Woolf went into Cornwall; therefore if such engines were now used at the Consolidated mines, the expense of drainage might be expected to average ($£13,826 \times 39{\cdot}36 \div 19{\cdot}38 =$) £28,100 per annum, or £14,274 more than it has been; and it is that saving which has constituted the whole of the profit made during the last five years.

During the last five years the mines have produced 73,561 tons of copper

* This does not include any valuation of the materials in use in the mines; although the cost of all materials is included in the amount of total expenditure. The materials would sell for a large sum, but that can scarcely be reckoned as a part of the profit, because from the uncertainty of mining prospects a mine cannot be given up in time to realize it: the working is usually continued at a loss until a debt is incurred, and when the adventurers become too much discouraged to make further advances, the mine is given up, and the materials sold to pay the debt.

copper ore, which on an average has yielded about $9\frac{1}{2}$ per cent. of copper and $36\frac{1}{2}$ tons of tin ore. The ores have been sold for £548,872, of which one twenty-fourth has been paid to the lord of the soil as rent. The total cost of working the mines has been £462,444; leaving a clear profit of £63,604.

The above is but a limited view of the advantages arising from the use of Mr. Woolf's engines; for if the mines had been begun in 1819 with Mr. Watt's engines, the loss during the first years would have been considerably greater than it was, and the mines would have continued to be unprofitable for a longer time than three years; also the subsequent profit would have been so much smaller than it has been, that it would not have repaid the previous loss (and interest upon the capital advanced) for a long time to come, beyond the present date, supposing the mines to continue to yield ore. In fact if these mines could have been worked with profit by Mr. Watt's engines, they would not have been given up as they were, twenty-five years ago, when they were not worked out so deep, or so extensively, as they are now.

Engines and Cost of Drainage at the Consolidated and United Mines.

Date.	No. of Engines.	Horse Power.	Average Millions.	Cost of Drainage.	Clear Profit.	Expense saved.
1825	4	432	31·04	£17776	£4169	£9824
1826	6	422	32·31	13543	7648	8337
1827	8	378	36·76	13426	13294	11254
1828	8	526	44·86	12428	22314	15452
1829	8	513	51·81	11958	16179	19042
Aver^e.	7	454	39·36	£13826	£12721	£12782

The last column of the table shows the saving that has been made by the use of Mr. Woolf's engines, or the increase in the cost of drainage that would have been incurred by using Mr. Watt's engines, raising twenty millions, instead of the engines actually used: those savings are included in and form part of the profits; and without them the extra expense would have absorbed more than all the profit in 1825, 1826 and in 1829; so that instead of profit, the adventurers would have lost £5655 in 1825, £689 in 1826, and £2863 in 1829. Or taking the whole of the last five years, no profit would have been made; and it would have been more advantageous to the adventurers to have broken up their establishment, and sold the materials, than to have continued working.

In conclusion, it may be safely asserted that the saving in fuel resulting from the general use of Mr. Woolf's system of working steam-engines by high-pressure steam acting expansively (instead of Mr. Watt's system of working them by low-pressure steam acting expansively), constitutes the present profits of deep mining in Cornwall.

37, Howland Street, Fitzroy Square, JOHN FAREY.
London, June 5, 1830.

CIRCULATED BY THE ASTRONOMICAL SOCIETY.

Principal Lunar Occultations of the Planets and fixed Stars in October 1830. *Computed for Greenwich by* T. HENDERSON, *Esq.*

1830.	Stars' Names.	Ast. Soc. Cat. No.	Mag.	Immersions.			Emersions.		
				Sidereal time.	Mean solar time.	Angle.	Sidereal time.	Mean solar time.	Angle.
				h m	h m	°	h m	h m	°
Oct. 1	10 Ceti	32	6	0 56	12 15	145	2 0	13 19	291
2	89 *f* Pisc.	139	6	21 15	8 31	20	21 47	9 3	318
5	48 Tauri	468	6	21 38	8 42	120	22 7	9 11	189
—	γ —	478	3·4	23 15	10 18	116	23 52	10 56	195
—	71 —	503	5·6	2 6	13 9	33	2 57	14 0	309
—	θ^1 —	510	5	3 14	14 17	91	4 26	15 29	281
—	θ^2 —	511	5·6	3 15	14 18	71	4 25	15 27	301
—	(99) —	516	5·6	4 27	15 30	136	5 29	16 31	266
—	85 —	520	6	5 12	16 15	53	5 48	16 50	363
—	Aldebaran	528	1	7 45	18 47	225	☽'s limb almost grazing; star occulted to places further south.		
6	111 Tauri	640	6	0 59	11 58	61	2 4	13 3	244
—	117 —	655	6	3 16	14 15	341	☽'s limb almost grazing; star occulted to places further north.		
11	31 A Leon.	1207	5	5 52	16 31	74	6 48	17 27	185
14	Venus			*6 20	16 47	342	7 1	17 28	264
23	43 *d* Sagitt.	2230	5	19 28	5 22	112	20 48	6 41	282
27	81 Aquarii	2748	6	1 19	10 56	175	2 11	11 48	285
30	ν Piscium	184	5	19 53	5 19	25	20 30	5 56	295
31	μ Ceti	293	4	21 40	7 2	9	22 8	7 29	313

The *angles* are reckoned from the vertex, towards the right hand, round the circumference of the moon's disc, as exhibited in an inverting telescope.

Apartments of the Astronomical Society,
57, Lincoln's Inn Fields, August 24, 1830.

AURORA BOREALIS.

In the evening of the 20th, between 8 and 9 o'clock, a bright light appeared in the northern horizon, and continued to rise and fall till twenty minutes past 9, when it occupied a space of 75 degrees; and at this time the first column of light, being thin and of a flame-colour, rose from its base nearly under Polaris, and was followed by many other columns from half to 3 degrees in width, some of which ascended upwards of 30 degrees. About 10 o'clock the greatest display of coruscations happened, when eight or ten distinct columns of light, nearly equidistant from each other, appeared at one view, and several small meteors showed themselves above the aurora, whose approach was indicated immediately after sunset by a fine rose-colour in the northern horizon, the same as the last was on the 19th of last April. During its continuance for three hours, the sky was

* At immersion, *in* or *under* the horizon.

perfectly

perfectly clear, except a dark haze, consisting of falling dew all round the horizon, about 5 degrees in altitude, which was particularly distinguished in the space occupied by the aurora.

LIST OF NEW PATENTS.

To J. Surman, Hounslow Barracks, Middlesex, lieutenant and riding-master in the Tenth Hussars, for certain improvements on bits for horses and other animals.—Dated the 6th of July, 1830.—2 months allowed to enrol specification.

To W. W. Tuxford, Boston, Lincolnshire, miller, for a machine or apparatus for cleansing or purifying wheat, grain or other substances.—6th of July.—6 months.

To Edw. Cowper, Streatham Place, Surrey, and Eben. Cowper, of Suffolk-street, Pall Mall East, Westminster, engineers, for certain improvements in printing-machines.—19th of July.—6 months.

To J. Rawe, Junior, Albany-street, Regent's Park, Middlesex, being one of the people called Quakers, and J. Boase, of the same place, gentleman, for certain improvements in steam carriages and in boilers, and a method of producing increase of draft.—19th of July.—6 months.

To T. Bulkeley, Albany-street, Regent's Park, Middlesex, M.D. for certain improvements in propelling vessels, which improvements are also applicable to other purposes.—19th of July.—6 months.

To W. Taylor, Wednesbury, Staffordshire, engineer, for certain improvements on boilers and apparatus connected therewith, applicable to steam-engines and other purposes.—19th of July.—6 mon.

To E. Riley, Skinner-street, Bishopsgate-street, brewer, for certain improvements in the process and apparatus for fermenting malt and other liquors.—19th of July.—6 months.

To G. Oldland, Hillsley, in the parish of Hawkesbury, Gloucestershire, clothworker, for certain improvements in the machinery or apparatus for shearing and dressing woollen cloths and other fabrics.—22d of July.—6 months.

J. Ericsson, New Road, Middlesex, engineer, for an improved engine for communicating power for mechanical purposes.—24th of July.—6 months.

To A. Garnett, Demerara, esquire, for certain improvements in manufacturing sugar.—24th of July.—6 months.

To S. Roberts, Park Grange, near Sheffield, silver-plater, for certain improvements in plating or coating of copper or brass, or mixture of the same, with other metal or materials, or with two metals or substances upon each other; as also a method of making such kind of articles or utensils with the said metal, when so plated, as have hitherto been made either entirely of silver, or of copper or brass, or of a mixture of copper and brass, plated or coated with silver solely.—26th of July.—2 months.

To R. Ibotson, Poyle, in the parish of Stanwell, Middlesex, paper manufacturer, for an improvement in the method or apparatus for separating the knots from paper stuff or pulp, used in the manufacture of paper.—29th of July.—4 months.

To

To J. Ruthven, Edinburgh, engineer and manufacturer, for an improvement in machinery for the navigating of vessels and propelling of carriages.—5th of August.—6 months.

To J. Down, Leicester, surgeon, for certain improvements in making gas for illumination, and in the apparatus for the same.—5th of August.—6 months.

To J. Street, Clifton, Gloucestershire, esquire, for a new mode of obtaining a rotary motion by water, steam, or gas, or other vapour; being applicable also to the giving blast to furnaces, forges, and other purposes where a constant blast is required.—5th of August.—2 months.

To W. Dobree, Fulham, Middlesex, gentleman, for an independent safety-boat of novel construction.—5th of August.—6 months.

To W. Lane, Stockport, Cheshire, cotton-manufacturer, for certain improvements in machines, which are commonly known among cotton-spinners by the names of roving-frames, or otherwise called cove-frames, or bobbin- and fly-frames, or jack-frames.—5th of August.—4 months.

METEOROLOGICAL OBSERVATIONS FOR AUGUST 1830.

Gosport:—Numerical Results for the Month.

Barom. Max. 30·32. Aug. 31. Wind N.W.—Min. 29·43. Aug. 28. Wind S.W.
Range of the mercury 0·89.
Mean barometrical pressure for the month .... 29·950
Spaces described by the rising and falling of the mercury............ 5·310
Greatest variation in 24 hours 0·500.—Number of changes 19.
Therm. Max. 75°. Aug. 1. Wind S.W.—Min. 44°. Aug. 29. Wind W.
Range 31°.—Mean temp. of exter. air 59°·79. For 31 days with ☉ in ♌ 62·81
Max. var. in 24 hours 21°·00.—Mean temp. of spring-water at 8 A.M. 52·77

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the atmosphere, in the afternoon of the 14th 91°
Greatest dryness of the atmosphere, on several days.................. 48·0
Range of the index .... 43·0
Mean at 2 P.M. 59°·8.—Mean at 8 A.M. 66°·9.—Mean at 8 P.M. 74·0
—— of three observations each day at 8, 2, and 8 o'clock .... 66·9
Evaporation for the month 3·50 inches.
Rain in the pluviameter near the ground 3·40 inches.
Prevailing wind, S.W.

Summary of the Weather.

A clear sky, 3; fine, with various modifications of clouds, 16½; an overcast sky without rain, 6; rain, 5½.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
22	11	30	1	24	29	20

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
1½	1½	½	2	1	10½	8	6	31

General

General Observations. — The first part of the month to the 8th was fine and dry, and the latter part alternately dry and showery. From the 8th to the 15th the weather was generally wet, which retarded the wheat harvest in this neighbourhood for a few days; but it was afterwards got in well, and all the barley and oats will be carried in the first week of September. From a perusal of the reports from all parts of the country respecting the harvest, it appears that the wheat crops in general have turned out excellent in quality and abundant in quantity beyond the most favourable expectations; and the barley and oat crops that have been carried in the southern parts of England, have yielded to the growers an extraordinary quantity, and are far superior in quality than for several years past. The same cheering account holds good with respect to Ireland and Scotland, which is somewhat surprising when we reflect upon the low temperature, and the uncommon vicissitudes of weather we have experienced this year:—but what necessary benefits cannot an All-bountiful Providence confer on man in due season?

Sheet lightning occurred in the evenings of the 4th, 17th, and 18th; and a thunder-storm passed over at 2 P.M. on the 13th, with forked lightning, which in the black *nimbus*, on which the sun shone brightly, had a grand appearance.

Early in the mornings of the 20th, 21st, and 30th, slight hoar frosts appeared in the grass-fields, which is rather unusual in August.

The mean temperature of the external air this month is about 3½ degrees under the mean of August for many years past.

The atmospheric and meteoric phænomena that have come within our observations this month, are one lunar and three solar halos, nine meteors, an aurora borealis; and eight gales of wind, or days on which they have prevailed, namely, one from the South-east, six from the South-west, and one from the West.

REMARKS.

London. — August 1. Fine. 2. Fine: heavy rain in the afternoon. 3. Showery. 4. Very warm: cloudy with showers: lightning at night. 5—9. Fine. 10. Dull, with slight rain. 11. Fine: rain at night. 12. Fine, with showers. 13. Heavy rain: thunder in the afternoon. 14. Cloudy. 15. Fine, with slight showers. 16. Fine. 17. Very fine in the morning: heavy rain at night. 18—21. Fine: cold at nights. 22. Very fine: rain. 23. Cloudy. 24. Fine: rain at night. 25. Cloudy: lightning and rain at night. 26. Fine. 27. Heavy rain in the afternoon, and at night. 28. Stormy and wet. 29. Thunder showers. 30, 31. Very fine.

Penzance.—August 1. Rain. 2. Fair. 3. Clear. 4. Fair: rain. 5, 6. Fair. 7. Rain: fair. 8. Showers: clear. 9. Fair: rain. 10. Fair. 11, 12. Fair: rain. 13. Fair: showers. 14. Rain: fair. 15, 16. Fair: showers. 17. Showery: clear. 18—21. Clear. 22, 23. Fair. 24. Fair: rain. 25. Clear: fair. 26. Showers: fair. 27. Rain. 28, 29. Showers. 30, 31. Clear.

Boston.—August 1. Cloudy: rain P.M. 2—4. Cloudy. 5. Fine. 6. Cloudy. 7. Fine. 8. Cloudy: rain P.M. 9. Fine: rain P.M. 10. Cloudy. 11. Cloudy: rain A.M.: rain at night. 12. Fine. 13. Cloudy: rain A.M. and P.M. 14, 15. Cloudy: rain P.M. 16. Fine. 17. Fine: rain P.M. 18. Fine: rain A.M. 19—21. Cloudy. 22. Cloudy: rain at night. 23. Fine: rain at night. 24. Cloudy. 25. Fine: rain at night. 26. Fine. 27, 28. Fine: rain at night. 29. Cloudy: rain early A.M. 30, 31. Fine.

Meteoro-

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1830.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.									
Aug. 1	29·994	29·822	29·85	29·83	30·07	29·95	29·27	78	54	65	58	75	59	68	s.	sw.	sw.	w.	...	...	0·265	0·015	...
2	29·906	29·812	29·84	29·80	29·98	29·90	29·14	73	53	65	58	69	58	63	w.	sw.	sw.	w.	...	0·23	...	...	0·06
3	30·106	29·981	30·00	29·98	30·13	30·08	29·32	75	53	68	55	71	59	63	sw.	w.	sw.	w.	0·40	·05	...	...	...
○ 4	30·049	29·878	29·90	29·90	30·08	29·95	29·50	80	60	66	58	73	62	60·5	se.	sw.	se.	calm	...	·01	·240	...	...
5	29·990	29·885	30·00	29·92	30·07	30·00	29·25	77	49	64	52	70	54	64	w.	w.	nw.	w.	...	...	...	...	...
6	30·021	30.002	30·05	30·00	30·10	30·09	29·34	68	52	64	56	68	56	59	w.	w.	w.	w.	·50	...	...	...	...
7	29·934	29·813	29·90	29·60	30·00	29·83	29·36	71	54	64	54	70	57	61	sw.	sw.	sw.	nw.	...	...	·180	·220	...
8	29·807	29·713	29·70	29·60	29·82	29·70	29·20	74	46	67	54	69	55	61·5	e.	se.	nw.	calm	...	...	...	...	...
9	29·809	29·758	29·80	29·70	29·84	29·77	29·22	76	55	67	55	70	58	62	s.	se.	se.	nw.	·30	...	·170	·150	·23
10	29·654	29·633	29·70	29·68	29·66	29·64	29·16	65	56	64	55	65	53	60	e.	n.	ne.	s.	...	·06	...	·420	·31
☾ 11	29·714	29·684	29·70	29·60	29·80	29·76	29·15	77	55	62	49	68	57	62·5	sw.	sw.	sw.	se.	...	·04	·660	·150	·02
12	29·802	29·746	29·72	29·70	29·90	29·80	29·09	72	55	65	55	68	57	62	sw.	w.	sw.	w.	·30	·07	·260	·360	·08
13	29·805	29·646	29·80	29·70	29·90	29·71	29·10	66	50	63	53	63	54	61	sw.	w.	w.	w.	...	·72	·250	·600	...
14	29·882	29·545	29·73	29·70	29·94	29·64	29·28	67	49	66	53	64	49	60	s.	s.	sw.	w.	...	·56	·370	·660	·10
15	29·872	29·743	29·90	29·80	29·90	29·81	29·10	70	44	62	53	63	49	58·5	w.	n.	w.	w.	·30	·01	·070	·070	·29
16	29·969	29·935	29·95	29·92	30·30	30·30	29·32	69	41	62	50	64	48	57	sw.	nw.	w.	w.	...	...	·110	·040	·10
17	29·967	29·734	30·00	29·90	30·05	30·00	29·37	68	44	58	50	63	48	54	w.	nw.	nw.	nw.	...	·53	·025	·110	...
● 18	30·199	30·157	30·15	30·10	30·22	30·18	29·54	64	40	62	48	62	48	54	ne.	ne.	n.	nw.	·30	...	...	...	·19
19	30·187	30·076	30·15	30·10	30·21	30·12	29·58	66	41	62	47	63	45	55	nw.	e.	nw.	n.	...	...	...	...	·03
20	29·986	29·961	30·02	30·00	30·07	30·01	29·38	64	47	62	48	62	45	52	nw.	n.	nw.	n.	...	...	...	...	...
21	30·008	29·973	30·00	30·00	30·07	30·06	29·43	70	39	66	50	63	48	54·5	w.	nw.	w.	n.	·40	...	...	...	...
22	30·030	30·017	29·98	29·94	30·09	30·07	29·46	74	66	65	56	68	57	55	w.	nw.	w.	w.	...	·01	...	·010	...
23	29·987	29·920	29·95	29·86	30·05	30·00	29·32	69	65	66	54	69	57	62	w.	w.	sw.	w.	...	...	...	...	·15
24	29·880	29·835	29·93	29·85	29·96	29·95	29·15	72	55	67	56	69	56	61·5	w.	w.	sw.	w.	·30	·02	·130	·035	·11
25	29·795	29·708	29·85	29·85	29·86	29·84	29·02	70	54	65	54	66	55	61	w.	nw.	sw.	w.	...	·17	...	...	...
☽ 26	29·871	29·835	29·85	29·83	29·95	29·90	29·16	69	49	60	55	66	53	56	w.	nw.	w.	w.	...	...	·150	·080	·08
27	29·764	29·404	29·60	29·40	29·82	29·44	29·16	71	53	60	54	66	55	57	sw.	sw.	s.	w.	·30	·40	·780	·250	...
28	29·611	29·291	29·65	29·50	29·64	29·43	28·73	66	48	59	54	64	52	60	w.	nw.	sw.	w.	...	·16	·700	·080	·17
29	30·100	29·911	30·00	29·90	30·10	29·93	29·24	68	40	60	49	65	44	55	sw.	nw.	w.	nw.	...	...	·010	...	·41
30	30·234	30·192	30·15	30·12	30·23	30·23	29·60	70	38	60	47	64	46	54	w.	n.	w.	nw.	...	...	...	·150	...
31	30·303	30·283	30·25	30·25	30·32	30·30	29·68	73	45	61	47	65	48	54	w.	ne.	nw.	nw.	·40	...	...	...	...
	30.303	29·291	30·25	29·40	30·32	29·43	29·27	80	38	68	47	75	44	58·9					3·50	3·04	4·370	3·400	2·33

THE
PHILOSOPHICAL MAGAZINE
AND
ANNALS OF PHILOSOPHY.

[NEW SERIES.]

NOVEMBER 1830.

XLVIII. *On the Problems of the Calculus of Variations. By* Hugh Ker Cankrien, *Esq. M.A. Trin. Coll. Cambridge.**

THERE are few students who do not find difficulty in the study of the calculus of variations. This does not arise so much from their want of familiarity with the notation made use of, since this is nearly the same as that of the differential calculus, but rather from their not having a distinct view presented to them at the outset, of the necessity for, and the object of, the several operations performed. It will hardly be denied that this is a fault of the method of Lagrange, when applied only to the solution of the more simple problems. His investigation contains the solution of the difficult as well as the easier problems, and consequently a great part of the process is unnecessary for the solution of the latter. For these reasons it is here attempted, with the assistance to be derived from M. Poisson's solution of the problem of the brachystochron, contained in the first volume of his treatise on Mechanics, to give first a solution of the easier problems, and then to show how far this solution is applicable to the more difficult, and in what way the solution of these may be completed.

The object proposed is to investigate the relation which the variables involved in a proposed function must have to one another, in order that a definite value of this given function shall be a maximum or minimum. The most common form in which this function is proposed, is the integral taken between limits of an expression containing the variables themselves and the differential coefficients of one of them considered as a function of the other. The limits are some-

* Communicated by the Author.

times invariable, and sometimes variable: the problem of the brachystochron is an instance of one or other of these two classes of problems, accordingly as we investigate the line of quickest descent from one given point to another given point, or from one given curve to another given curve.

If then $V = f(x, y, p, q, r, \&c.)$ where $p = \frac{dy}{dx}$; $q = \frac{d^2y}{dx^2}$; $r = \frac{d^3y}{dx^3}$; &c.; $U = \alpha + \int dx V$; α being an arbitrary constant; and $U_{,} - U_{,,}$ be the value of U between the limits $x_{,}y_{,}$; $x_{,,}y_{,,}$: our object is to find the relation between x and y, which renders $U_{,} - U_{,,}$ a maximum or minimum; either when $x_{,}y_{,}$, $x_{,,}y_{,,}$ are invariable, or when they are variable and connected by given equations $y_{,} = \phi(x_{,})$, $y_{,,} = \psi(x_{,,})$.

Now it is to be observed, that if we can determine the form of the function $F(x)$ in the equation $y = F(x)$, which renders $U_{,} - U_{,,}$ a maximum or minimum, when the limits are invariable; we can also find it by the same process, when the limits are not assigned, but when equations only are given connecting them: for we may suppose that the symbols $x_{,}y_{,}$, $x_{,,}y_{,,}$, which represented the limits which are supposed invariable in that process, now represent those values of the variables in the equations $y_{,} = \phi(x_{,})$, $y_{,,} = \psi(x_{,,})$ which must be taken as the limits of the integral in order that $U_{,} - U_{,,}$ shall be a maximum or minimum. The only difference in the results we shall obtain in the two cases will be this: when the limits are assigned, we can substitute their given values for the symbols $x_{,}y_{,}$, $x_{,,}y_{,,}$, and thus find determined values for one or more of the constants in the equation $y = F(x)$; whereas when we have not the values of the limits assigned, those constants which may be expressed in terms of these symbols will continue arbitrary, unless we have some method of determining the values of the limits. We will then for the present consider the limits of the integral not to change.

We supposed $U = \alpha + \int dx V$ when the function of x represented by y is involved in V: if $y = F(x)$ then $U_{,} - U_{,,}$ is the maximum or minimum value of $\int dx V$ when taken between the limits $x_{,}y_{,}$ and $x_{,,}y_{,,}$. Let u be any function of x which vanishes when $x_{,}$ or $x_{,,}$ is substituted in it for x; and let k be a very small constant quantity; also let W be the value of $\alpha + \int dx V$ when $y + ku$ is substituted in V for y. Then $W_{,} - W_{,,}$ is greater or less than $U_{,} - U_{,,}$ according as $U_{,} - U_{,,}$ is a minimum or a maximum, whatever be the function u. Now, when $y + ku$ is substituted for y in p, it becomes $p + k\frac{du}{dx} = p + ku'$; q becomes $q + ku''$; r becomes $r + ku'''$; &c.; and therefore since W is the value of U when $y + ku$ is substituted

tuted for y in V; $p + k u'$ for p; $q + k u''$ for q; $r + k u'''$ for r; &c.; we have

$$W = U + k \int dx \left\{ \frac{dV}{dy} u + \frac{dV}{dp} u' + \frac{dV}{dq} u'' + \frac{dV}{dr} u''' + \&c. \right\}$$

$$+ \frac{k^2}{2} \int dx \left\{ \frac{d^2V}{dy^2} u^2 + 2 \frac{d^2V}{dy\, dp} u u' + \frac{d^2V}{dp^2} u'^2 + 2 \frac{d^2V}{dy\, dq} u u'' \right.$$

$$\left. + 2 \frac{d^2V}{dp\, dq} u' u'' + \frac{d^2V}{dq^2} u''^2 + \&c. \right\} + \&c.$$

$$= U + k \int dx \left\{ N u + P u' + Q u'' + R u''' + \&c. \right\} +$$

$$\frac{k^2}{2} \int dx \{ A u^2 + 2 B u u' + C u'^2 + 2 D u u'' + 2 E u' u'' +$$

$F u''^2 + \&c.\} + \&c.$ for the sake of conciseness.

Now $\int dx\, P u' = P u - \int dx\, u P'$: and therefore, since u vanishes at the limits, $\int dx\, P u'$ is the same as $- \int dx\, u P'$, when both are taken beween the limits. Hence if we denote the $\int dx\, P u'$ when taken between the limits by $\int_{x_{\prime\prime}}^{x_{\prime}} dx\, P u'$, we have $\int_{x_{\prime\prime}}^{x_{\prime}} dx\, P u' = - \int_{x_{\prime\prime}}^{x_{\prime}} dx\, u P'$. Similarly we shall find $\int_{x_{\prime\prime}}^{x_{\prime}} dx\, u'' Q = \int_{x_{\prime\prime}}^{x_{\prime}} dx\, u Q''$: and so on*. Hence if we substitute the preceding results, we find

$$W_{\prime} - W_{\prime\prime} = U_{\prime} - U_{\prime\prime} + k \int_{x_{\prime\prime}}^{x_{\prime}} dx\, \{ N - P' + Q'' - R''' + \&c. \}$$

+ &c. the terms following those which are written down being multiplied by k^2, k^3, &c. Now since the term multiplied by k may be altered from positive to negative by only

* It may be necessary to explain the notation made use of in the following investigations. The expressions $\int dx\, V$ and $\int dx\, \{N - P' + Q'' - \&c.\}$ denote the functions whose differential coefficients taken with respect to x are V and $N - P' + Q'' - \&c.$ respectively: $\int dx$ is considered in these expressions as a mere symbol. For the sake of conciseness it is desirable to have an expression for such a function as $\int dx\, V$ when a certain value, as $x_{\prime}$ or $x_{\prime\prime}$, is assigned to the variable x contained in it: the expressions $\int^{x_{\prime}} dx\, V$ and $\int^{x_{\prime\prime}} dx\, V$ are used for this purpose. An expression equivalent to $\int^{x_{\prime}} dx\, V - \int^{x_{\prime\prime}} dx\, V$ very often occurs, and it is therefore convenient to express it by one term, as $\int_{x_{\prime\prime}}^{x_{\prime}} dx\, V$. In the use made of these expressions it will be seen that $\int^{x_{\prime}} dx$ and $\int_{x_{\prime\prime}}^{x_{\prime}} dx$ are considered as mere symbols.

changing

changing the sign of k, and, by properly assuming k, may be made greater than the sum of all the terms which follow it, if $U_{,}-U_{,,}$ be either a maximum or a minimum, it is necessary that $N - P' + Q'' - R''' + \&c. = 0$. This equation when integrated will give us the relation between x and y, or the equation $y = F(x)$ which renders $\int_{x_{,,}}^{x_{,}} dx\,V$ a maximum or a minimum.

The order of the differential equation we have just found will in general be $2n$, if the order of the highest differential coefficient in V be n. We may find an equation one order lower in this way:

$$\frac{d\,(V)}{dx} = V' = \frac{dV}{dx} + \frac{dV}{dy}\,\frac{dy}{dx} + \frac{dV}{dp}\,\frac{dp}{dx} + \frac{dV}{dq}\,\frac{dq}{dx} + \frac{dV}{dr}\,\frac{dr}{dx} + \&c.;$$

or, $\quad V' = M + Np + Pq + Qr + Rs + \&c.$

but $\quad 0 = -p\,\{N - P' + Q'' - R''' + \&c.\}$ by the equation last found.

$$\therefore V' = M + Pq + P'p + Qr - pQ'' + Rs + R'''p + \&c.$$

Now, $Pq + pP' = (Pp)'$; $Qr - pQ'' = Qr + Q'q - Q'q - Q''p = (Qq - pQ')'$; $Rs + R'''p = Rs + rR' - rR' - qR'' + qR'' + pR''' = (Rr - qR' + pR'')'$; &c. = &c.: substituting these results we find

$$V' = M + (Pp)' + (Qq - pQ')' + (Rr - qR' + pR'')' + \&c., \text{ and}$$

$$\therefore V = \beta + \int dx\,M + Pp + Qq - pQ' + Rr - qR' + pR'' + \&c. \quad (1).$$

This equation contains one arbitrary constant β: and if the order of the highest differential coefficient in V be n, there will in general be $2n$ arbitrary constants in the primitive equation between x and y. These are to be determined by means of the limits and other data which, according to the case, must be granted for that purpose. But if the limits are not given, the constants which are expressed in terms of the symbols representing them, must remain arbitrary, as was observed above. Our object then, in order to complete the solution in this case, must be to determine the actual values of the limits which these symbols represent.

Since $y_{,} = \varphi(x_{,})$ and $y_{,,} = \psi(x_{,,})$ we may consider $U_{,}-U_{,,}$ as a function of $x_{,}$ and $x_{,,}$: now if we substitute $x_{,} + \delta x_{,}$ for $x_{,}$ and $x_{,,} + \delta x_{,,}$ for $x_{,,}$ in $U_{,}-U_{,,}$, the result, which we will denote by $\omega_{,}-\omega_{,,}$, is $U_{,}-U_{,,} + \mu\,\delta x_{,} + \pi\,\delta x_{,,} + \&c.$, μ and π representing the coefficients of $\delta x_{,}$ and $\delta x_{,,}$ in the expanded expression, and since $U_{,}-U_{,,}$ is always greater or always less

less than $\omega_{,}-\omega_{,,}$ we must have $\mu\,\delta x_{,} + \pi\,\delta x_{,,} = 0$ by the common theory.

This equation $\mu\,\delta x_{,} + \pi\,\delta x_{,,} = 0$ resolves itself into two others, since $\delta x_{,}$ and $\delta x_{,,}$ are independent of one another, in which $x_{,}$ and $x_{,,}$ are the only unknown quantities: they therefore may be determined by means of them.

Now $\omega_{,}-\omega_{,,}$ being the value of $U_{,}-U_{,,}$ when $x_{,}+\delta x_{,}$ is substituted for $x_{,}$ and $x_{,,}+\delta x_{,,}$ for $x_{,,}$ in it; and since $x_{,}$ and $x_{,,}$ enter into $U_{,}$ and $U_{,,}$ respectively, partly in consequence of their being involved in U, and partly in consequence of the change of x into $x_{,}$ and $x_{,,}$ respectively wherever x occurs in U; it is evident that $\omega_{,}$ may be obtained by substituting $x_{,} + \delta x_{,}$ for $x_{,}$ and $x_{,,} + \delta x_{,,}$ for $x_{,,}$ wherever they occur in U, and then substituting $x + \delta x$ for x, and afterwards changing x and δx into $x_{,}$ and $\delta x_{,}$ in the result. In like manner we may find $\omega_{,,}$, or the value of $U_{,,}$ when $x_{,} + \delta x_{,}$ and $x_{,,} + \delta x_{,,}$ are substituted for $x_{,}$ and $x_{,,}$ in it, by first substituting $x_{,}+\delta x_{,}$ and $x_{,,}+\delta x_{,,}$ for $x_{,}$ and $x_{,,}$ wherever they occur in U, and then substituting $x + \delta x$ for x, and afterwards changing x and δx into $x_{,,}$ and $\delta x_{,,}$. All these substitutions are to be made not only where $x_{,}$ and $x_{,,}$ occur alone, but likewise where they occur involved in the functions $y_{,}$ and $y_{,,}$. In like manner since the operation of substituting $x + \delta x$ for x in U is equivalent to the substitution of $x_{,} + \delta x_{,}$ for $x_{,}$, when $\omega_{,}$ is obtained; and equivalent to the substitution of $x_{,,} + \delta x_{,,}$ for $x_{,,}$, when $\omega_{,,}$ is sought; it is evident that in one case y must be considered as the same function of x that $y_{,}$ is of $x_{,}$; and in the other y must be considered the same function of x that $y_{,,}$ is of $x_{,,}$.

Since we only want those terms which are multiplied by the first powers of $\delta x_{,}$ and $\delta x_{,,}$, we will carry the operations in what follows only to that extent. We will also use $\delta y_{,}$ to denote the term multiplied by $\delta x_{,}$ in the new value of $y_{,}$ obtained by substituting $x_{,} + \delta x_{,}$ for $x_{,}$ in it. In like manner $\delta y_{,,}$ denotes the term multiplied by $\delta x_{,,}$ in the new value of $y_{,,}$ when $x_{,,}+\delta x_{,,}$ is substituted for $x_{,,}$ in $y_{,,}$; and δy denotes the corresponding term when $x+\delta x$ is substituted, for x in y considered as a function of x: δp the term in p: and so on.

Now, if U contain $x_{,}\, y_{,}\, x_{,,}\, y_{,,}$, and we substitute $x_{,} + \delta x_{,}$ for $x_{,}$ and $x_{,,}+\delta x_{,,}$ for $x_{,,}$, the new value of U thus obtained is

$$U + \frac{d\,U}{d\,x_{,}}\,\delta x_{,} + \frac{d\,U}{d\,y_{,}}\,\delta y_{,} + \frac{d\,U}{d\,x_{,,}}\,\delta x_{,,} + \frac{d\,U}{d\,y_{,,}}\,\delta y_{,,} + \&c.$$

but $U = \alpha + \int d\,x\,V$, and α may be considered either as a function of $x_{,}\,y_{,}$ or of $x_{,,}\,y_{,,}$: we will suppose α to be a function

function of $x_{,}y_{,}$, and V to contain $x_{,}y_{,}x_{,,}y_{,,}$, and our expression becomes

$$U + \frac{d\alpha}{dx_{,}}\delta x_{,} + \frac{d\alpha}{dy_{,}}\delta y_{,} + \delta x_{,}\int dx\frac{dV}{dx_{,}} + \delta y_{,}\int dx\frac{dV}{dy_{,}} + \delta x_{,,}\int dx\,\frac{dV}{dx_{,,}} + \delta y_{,,}\int dx\,\frac{dV}{dy_{,,}} + \&c. \qquad (2)$$

Again, in order to obtain $\omega_{,}$ we must, as was remarked, substitute $x + \delta x$ for x in this expression (2), and then change x and δx into $x_{,}$ and $\delta x_{,}$: as we are only in want of the terms which are multiplied by the simple power of $\delta x_{,}$ and $\delta x_{,,}$, it will be sufficient to make this substitution in U. Now we found by (1) above

$$V = \beta + \int dx\,M + Pp + Qq - p\,Q' + \&c.$$

and, $\therefore U = \alpha + \int dx\,V = \alpha + \int dx\,\{\beta + \int dx M\} + \int dx\,p\,(P - Q') + \int dx\,q\,Q + \&c.$

$$= \alpha + \int dx\,\{\beta + \int dx M\} + y\,(P - Q') - \int dx\,y\,(P - Q')' + p\,Q - \int dx\,p\,Q' + \&c.$$

Let δU denote that part of the new value of U, when $x + \delta x$ is substituted for x in this expression, which is multiplied by the simple power of δx: then

$$\delta U = \delta x\{\beta + \int dx\,M\} + \delta y\,(P - Q') + y\,(P - Q')'\,\delta x - \delta x\,.\,y\,(P - Q')' + \delta p\,.\,Q + p\,Q'.\,\delta x - \delta x\,.\,p\,Q' + \&c.$$

$$= \delta x\,\{\beta + \int dx\,M\} + \delta y\,(P - Q') + \delta p\,.\,Q + \&c.$$

but, $V\,\delta x = \delta x\,\{\beta + \int dx\,M\} + p\,\delta x\,(P - Q') + q\,\delta x\,.\,Q + \&c.$ by (1)

$$\therefore \delta U = V\,\delta x + (\delta y - p\,\delta x)\,(P - Q' + \&c.) + (\delta p - q\,\delta x)\,(Q - \&c.) + \&c.$$

If then we substitute in (2) and then change δx and δy into $\delta x_{,}$ and $d y_{,}$, we find for the value of $\omega_{,}$

$$\frac{d\alpha}{dx_{,}}\delta x_{,} + \frac{d\alpha}{dy_{,}}\delta y_{,} + \delta x_{,}\int^{x_{,}} dx\,\frac{dV}{dx_{,}} + \delta y_{,}\int^{x_{,}} dx\,\frac{dV}{dy_{,}} + \delta x_{,}\int^{x_{,}} dx\,\frac{dV}{dx_{,,}} + \delta y_{,,}\int^{x_{,}} dx\,\frac{dV}{dy_{,,}} + \&c. + U_{,} + V_{,}\delta x_{,} + (\delta y_{,} - p_{,}\delta x_{,})\{P_{,} - Q_{,}' + \&c.\} + (\delta p_{,} - q_{,}\delta x_{,})\,\{Q_{,} - \&c.\} + \&c.$$

In this expression $\int^{x_{,}} dx\,\frac{dV}{dx_{,,}}$ denotes the value of $\int dx\,\frac{dV}{dx_{,,}}$ when $x_{,}$ is substituted for x in it. In like manner we shall find

$\omega_{,,} =$

$$\omega_{,,} = \frac{d\,\omega}{d\,x_,}\,\delta\,x_, + \frac{d\,\omega}{d\,y_,}\delta y_, + \delta\,x_,\int^{x_{,,}} d\,x \;\; \frac{d\,V}{d\,x_,} + \delta\,y_,\int^{x_{,,}} d\,x\,\frac{d\,V}{d\,y_,} +$$

$$\delta\,x_{,,}\int^{x_{,,}} d\,x\,\frac{d\,V}{d\,x_{,,}} + \delta\,y_{,,}\int^{x_{,,}} d\,x \;\; \frac{d\,V}{d\,y_{,,}} + \text{\&c.} + U_{,,} + V_{,,}\,\delta\,x_{,,} +$$

$$(\delta y_{,,} - p_{,,}\delta x_{,,})\{P_{,,} - Q_{,,}' + \text{\&c.}\} + \{\delta p_{,,} - q_{,,}\delta x_{,,}\}\{Q_{,,} - \text{\&c.}\} + \text{\&c.}$$

and therefore subtracting this result from the former, we find

$$\omega_, - \omega_{,,} = \delta\,x_,\int_{x_{,,}}^{x_,} d\,x\,\frac{d\,V}{d\,x_,} + \delta\,y_,\int_{x_{,,}}^{x_,} d\,x\,\frac{d\,V}{d\,y_,} + \delta\,x_{,,}\int_{x_,}^{x_,} d\,x\,\frac{d\,V}{d\,x_{,,}}$$

$$+ \delta\,y_{,,}\int_{x_{,,}}^{x_,} d\,x\,\frac{d\,V}{d\,y_{,,}} + \text{\&c.} + V_,\,\delta\,x_, - V_{,,}\,\delta\,x_{,,} + (\delta y_, - p_,\,\delta\,x_,)$$

$$(P_, - Q_,' + \text{\&c.}) - (\delta y_{,,} - p_{,,}\,\delta\,x_{,,})\,(P_{,,} - Q_{,,}' + \text{\&c.}) + U_, - U_{,,}$$
$$+ (\delta p_, - q_,\,\delta\,x_,)\,(Q_, - \text{\&c.}) - (\delta\,p_{,,} - q_{,,}\,\delta\,x_{,,})\,(Q_{,,} - \text{\&c.}) + \text{\&c.}$$

since $\int^{x_,} d\,x\,\frac{d\,V}{d\,x_{,,}} - \int^{x_{,,}} d\,x\,\frac{d\,V}{d\,x_{,,}} = \int_{x_{,,}}^{x_,} d\,x\,\frac{d\,V}{d\,x_{,,}}$; &c. = &c.

It appears, then, that the equation which we represented by $\mu\,\delta\,x_, + \pi\,\delta\,x_{,,} = 0$ is

$$\delta\,x_,\int_{x_{,,}}^{x_,} d\,x\,\frac{d\,V}{d\,x_,} + \delta\,y_,\int_{x_{,,}}^{x_,} d\,x\,\frac{d\,V}{d\,y_,} + \delta\,x_{,,}\int_{x_{,,}}^{x_,} d\,x\,\frac{d\,V}{d\,x_{,,}} +$$

$$\delta y_{,,}\int_{x_{,,}}^{x_,} d\,x\,\frac{d\,V}{d\,y_{,,}} + \text{\&c.} + V_,\delta x_, - V_{,,}\delta x_{,,} + (\delta y_, - p_,\delta x_,)\,(P_, - Q_,' + \text{\&c.})$$
$$-(\delta\,y_{,,} - p_{,,}\,\delta\,x_{,,})\,(P_{,,} - Q_{,,}' + \text{\&c.}) + (\delta p_, - q_,\,\delta\,x_,)\,(Q_, - \text{\&c.})$$
$$-(\delta\,p_{,,} - q_{,,}\,\delta\,x_{,,})\,(Q_{,,} - \text{\&c.}) + \text{\&c.} = 0 \qquad (3)$$

If V contains none of the quantities $x_, y_, x_{,,} y_{,,}$, then since $\frac{d\,V}{d\,x_,} = 0$ $\frac{d\,V}{d\,y_,} = 0$ $\frac{d\,V}{d\,x_{,,}} = 0$ and $\frac{d\,V}{d\,y_{,,}} = 0$, the equation for determining the limits is

$$V_,\,\delta\,x_, - V_{,,}\,\delta\,x_{,,} + (\delta\,y_, - p_,\,\delta\,x_,)\,(P_, - Q_,' + \text{\&c.}) -$$
$$(\delta\,y_{,,} - p_{,,}\delta x_{,,})\,(P_{,,} - Q_{,,}' + \text{\&c.}) + (\delta p_, - q_,\,\delta\,x_,)\,(Q_, - \text{\&c.}) -$$
$$(\delta\,p_{,,} - q_{,,}\,\delta\,x_{,,})\,(Q_{,,} - \text{\&c.}) + \text{\&c.} = 0 \qquad (4)$$

By means of this equation, then, we can determine the limits when they are not involved in V; but when they are involved in V, we must use equation (3), and therefore in either case we can determine the arbitrary constants as was required.

We will now resume the consideration of the value of $W_, - W_{,,}$, which in consequence of the preceding results is reduced to

$$U_, - U_{,,} + \frac{k^2}{2}\int_{x_{,,}}^{x_,} d\,x\,\{A\,u^2 + 2\,B\,u\,u' + C\,u'^2 + 2\,D\,u\,u'' +$$
$$2\,E\,u'\,u'' + F\,u''^2 + \text{\&c.}\} + \text{\&c.}$$

Now,

Now, $\int dx\, 2\, B u u' = \int dx\, B\, \frac{d u^2}{d x} = B u^2 - \int d x\, u^2 B'$, and therefore $\int_{x_{\prime\prime}}^{x_\prime} d x\, 2\, B u u' = - \int_{x_{\prime\prime}}^{x_\prime} d x\, u^2 B'$.

Similarly $\int_{x_{\prime\prime}}^{x_\prime} d x\, 2\, E u' u'' = - \int_{x_{\prime\prime}}^{x_\prime} d x\, E u'^2$.

Again, $\int d x\, 2\, D u u'' = D \frac{d u^2}{d x} - \int d x\, 2\, D u'^2 - D' u^2 + \int d x\, u^2 D''$, and therefore $\int_{x_{\prime\prime}}^{x_\prime} d x\, 2\, D u u'' = \int_{x_{\prime\prime}}^{x_\prime} d x\, u^2 D'' - \int_{x_{\prime\prime}}^{x_\prime} d x\, 2\, D u'^2$; and so on. Hence

$$W_\prime - W_{\prime\prime} = U_\prime - U_{\prime\prime} + \frac{k^2}{2} \int_{x_{\prime\prime}}^{x_\prime} d x \{ (A - B' + D'') u^2 + (C - 2 D - E') u'^2 + F u''^2 + \&c. \} + \&c.$$

Now, in order that $U_\prime - U_{\prime\prime}$ may be a maximum or a minimum, the term multiplied by k^2 in the above expression must be either positive or negative whatever be the function u; and $U_\prime - U_{\prime\prime}$ will be a maximum or a minimum according as this term is negative or positive. It is not easy to find generally the relation of the coefficients of u^2, u'^2, u''^2, &c. to one another by which these conditions are fulfilled. In order to determine it in particular cases we may remark, that if the values of $\Phi'(x)$ be found corresponding to the series of values $x_{\prime\prime}$, $x_{\prime\prime} + h$, $x_{\prime\prime} + 2h$, &c. $x_{\prime\prime} + \overline{n-1}\,.\,h$; h being $= \frac{x_\prime - x_{\prime\prime}}{n}$ and n a large number; then $\Phi(x_\prime) - \Phi(x_{\prime\prime})$ is positive or negative, according as the sum of the positive values of $\Phi'(x)$ found in the manner just mentioned, is greater or less than the sum of the negative values. If all these values of $\Phi'(x)$ are positive, or all negative, then $\Phi(x_\prime) - \Phi(x_{\prime\prime})$ is in the one case positive, and in the other negative, as is evident. If then we deduce the values of A, B, C, D, E, F, &c. from V, and the expression $(A - B' + D'') u^2 + (C - 2D - E') u'^2 + F u''^2 + \&c.$, which we will call $\Phi'(x)$, be reduced as much as possible by means of the equation $N - P' + Q'' - R''' + \&c. = 0$, we shall in many cases see whether it be possible so to assume the function u that $\frac{k^2}{2} \int_{x_{\prime\prime}}^{x_\prime} d x\, \Phi'(x)$ shall be either positive or negative: if this be possible, then the equation $N - P' + Q'' - R'' + \&c. = 0$ does not make $\int_{x_{\prime\prime}}^{x_\prime} d x\, V$ either a maximum or a minimum:

mum: but if it be not possible, it does. Thus if $V = \frac{\sqrt{1+p^2}}{\sqrt{2g}\sqrt{y-y_{\prime\prime}}}$: we find $\sqrt{y-y_{\prime\prime}} \cdot \sqrt{1+p^2} = c$, and by means of this equation we can show that $\Phi'(x)$ is always positive whatever be the function u. Thus we can in this case show analytically that we have obtained that relation between x and y which renders $\int_{x_{\prime\prime}}^{x'} d\,x\,V$ a minimum; and in a similar manner we can solve several other problems. The only case in which it is obvious that the general formula for $\Phi'(x)$ cannot change its sign, is when the coefficients of $u^2\ u'^2\ u''^2$ &c., are all of them either positive or negative for all values of x between the limits of the integral.

Lincoln's Inn, July 10th, 1830.

XLIX. *Remarks on a Passage in* Dr. Thomson's "*Outline of the Sciences of Heat and Electricity,*" *London*, 1830. *By the Rev.* B. Powell, *M.A. F.R.S., Savilian Professor of Geometry, Oxford.**

IN looking into the volume lately published by Dr. Thomson on Heat &c., my attention was immediately drawn to the chapter on radiant heat, as being a subject on which I have been particularly engaged. And I cannot but feel indebted to the distinguished author for the notice he has been pleased to take of my researches on this subject: though at the same time I trust he will allow me to make a few remarks on the mode in which the mention of them is introduced. The passage referred to is as follows:

"The conclusions from the observations of De la Roche have been called in question by Mr. Powell. He admits that when a hot body becomes luminous it gives out *heat* capable of passing directly through transparent screens. But this new heat acts more on a smooth black surface than on an absorptive white one. From this he concludes, that it is different from common radiant heat. We have no evidence that it is the same as light. It is great from red-hot metals, though the light be feeble. It exists in the solar rays, and is what produces the photometrical effect in Leslie's Photometer. But this ingenious explanation of Mr. Powell has, I think, been obviated by a very happy and instructive experiment of Mr. Ritchie," &c. &c. p. 156. Then follows a description of Mr. Ritchie's experiments in detail.

Now to a reader not previously acquainted with the sub-

* Communicated by the Author.

ject, it would certainly appear, from this whole passage, as if my investigations had consisted merely in making objections to De la Roche's conclusions, and suggesting another explanation of his facts; and that this explanation had been subsequently opposed by the experiments of Mr. Ritchie, and, in the opinion of the author, invalidated by them.

Such, it appears to me, is clearly what the *language* of the passage would *imply*. And if so, I must be permitted to say, the impression conveyed is very erroneous.

In the first place, as to my own researches, I have only to hope, that, in order to form a fair judgement of them, the reader will take the trouble of referring to the Phil. Trans. 1825. Part i., and 1826. Part iii., where he will perceive that my conclusions are advanced as the result of experiment, and, as such, bear the character of demonstrated fact. If they are to be refuted, it must be by pointing out some fallacy in the experiments, or by exhibiting others more accurate and satisfactory in opposition to them; and this, as far as I am aware, has not yet been done. If the facts be admitted, the explanation of De la Roche's results, and indeed of numerous others, follows as a necessary consequence.

I was very much surprised, that, in the passage referred to, the experiments and conclusions of Mr. Ritchie should be described as contradictory to mine, and invalidating my explanation of De la Roche's; when in fact, they do not even refer to the same subject as mine, except only on one subordinate point; and that, one which has no reference to De la Roche's researches.

In the main portion of my experiments, what I have stated respecting glass screens is certainly proved only for screens of ordinary and sensible thickness. If an exception were found when an extremely thin screen is employed, it would not interfere with the conclusion in other cases.

Such an exception has been contended for by Mr. Ritchie, on the ground of some very delicate experiments. I failed in verifying his conclusion. He objected that my trial was not sufficiently delicate; and adopted the ingenious variations upon his method which Dr. Thomson has described, confirming his original conclusion. Here this subordinate question rests. We have at present only to state the general law of non-transmissibility, with this single exception in the case of infinitely [indefinitely?] thin screens.

But this conclusion has manifestly no connection whatever with De la Roche's results; nor with my explanation, nor any other which may be given of them; this particular case being one which did not enter at all into his inquiries.

I must

I must trust to Dr. Thomson's candour to excuse the freedom of these remarks: and I must repeat, that nothing can be further from my intention than to charge him with misrepresentation: my only object being to guard his readers against the interpretation which I think would most naturally be put upon his words; and which would tend to convey a partial and confused view of the case he meant to state.

Oxford, Oct. 8th, 1830.

L. *On the probable Connection of Rock-Basins, in Form and Situation, with an internal concretionary Structure in the Rocks on which they occur: introduced by Remarks on the alleged Artificial Origin of those Cavities. By* E. W. Brayley, *Jun., A.L.S., Teacher of the Physical Sciences in the Schools of Hazelwood and Bruce Castle.**

THE basis of the following observations has already recently appeared, in a "Selection of Facts," illustrative of the Geology of Devonshire, which has been inserted in the Rev. T. Moore's new "History and Topography" of that county. The main objects, however, of a topographical publication, will necessarily limit the circulation of that work to certain classes of readers; and many persons who may take an interest in the natural history of Rock-basins, might not expect to find a discussion upon it in the pages of a County History. On these accounts, the author ventures to republish his inquiries on the subject, in the form of a separate memoir, with such corrections and additions as subsequent reflection and reading have suggested.

Mr. Moore, in a chapter of his work which is allotted to the subject of the original population &c. of Devonshire, has entered at large into the history of Druidism, as having been the religion of the Danmonii; who were either the aborigines or the early Belgic invaders of the south of Devonshire. In commencing this history, he enters into an examination of the proofs "that the Druids abounded in Devonshire, and were particularly conversant with Dartmoor and its vicinity," which have been conceived to be found in the existence of the Cromlech, Logan-stones, Rock-basins, and similar objects, upon Dartmoor and in the surrounding tract. After some discriminatory observations on the value of the evidence thus

* From "Outlines of the Geology, Physical Geography, and Natural History of Devonshire;" inserted in the Rev. T. Moore's "History and Topography of the County of Devon" now publishing: with corrections and additions by the author; and communicated by him.

thus supposed to be afforded, he makes the subjoined remarks on Rock-basins:

"Some of the rock-basins may be excavations produced by natural causes, but the form of others is much too regular, and affords indications of design which leave no room to doubt of their being artificial; nor is it easy to imagine from what their origin could be derived, if not from Druidical superstition. The notion of Dr. Macculloch and others, that rock-basins have been formed by the action of water, air and frost on the softer parts of the stone in all instances, seems to be entirely unfounded; for, if such were the case, how comes it to pass that they are found only on the tops of the tors, and sometimes on the logan-stones? This, if the writer is not mistaken, is a singular fact, and tends to strengthen the idea that these tors and logan-stones were appropriated by the Druids to their religious rites. If the basins were excavations produced by natural causes, why are they not found on numerous other rocks and in different situations? This circumstance, in conjunction with their regularity and the peculiar form common to many of them, seems to leave little room for scepticism*."

An impression, however, had been made upon the mind of the present writer, by Dr. Macculloch's arguments on this subject†, and confirmed by the observations he had enjoyed an opportunity of making on the very spot regarded by Dr. Borlase as having been the grand centre of Druidical worship,—Carnbrea Hill, in Cornwall—which was left unshaken by the perusal of the foregoing remarks. He conceived, therefore, that he should be liable to the charge of indifference to the interests of scientific truth, were he to refrain, when mentioning the Rock-basins of Devonshire, from stating and confirming the conclusions at which Dr. Macculloch had arrived. While, on the other hand, to leave those remarks unnoticed, might have seemed culpable inattention to the opinion of his friend and coadjutor Mr. Moore. Accordingly, in a section of the work allotted to the mineralogical characters, structure, and external configuration of the granite of Dartmoor, he introduces the following examination of Mr. Moore's views, succeeded by some further remarks on the geological history of the subject.

The regularity of form of the rock-basins, which is adduced, by Mr. Moore, as a proof of their being artificial, is

* Hist. and Topog. of the County of Devon, Book II. Chap. i.; Octavo Edition, vol. i. p. 106.

† See Dr. Macculloch's paper "On the Granite Tors of Cornwall," Trans. of Geol. Soc. 1st Series, vol. ii. p. 72.

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a consequence of the tendency of the action which produced them, to extend itself equally in every direction; which "the uniform texture of the granite" (to which, merely, it is ascribed by Dr. Macculloch,) would necessarily permit it to do*. Mr. Moore also brings forward, as militating against the truth of Dr. Macculloch's explanation, the facts that the rock-basins "are found only on the tops of the tors, and sometimes on the logan-stones:" and, resuming this argument, he inquires, "If the basins were excavations produced by natural causes, why are they not found on numerous other rocks and in different situations?" In reply to this it may be stated, that, in the granite of the Scilly Islands, and in the millstone-grit of Ashover in Derbyshire, (which is associated with the coal-measures, and belongs, consequently, to a very different group of rocks,) these basins do actually occur on the perpendicular sides of the rocks. That they should be sometimes found on the logan-stones, is also a consequence of their origin from natural causes; since the same action of the elements, which, operating on the angles and edges of the blocks of granite, has produced the logans—which Mr. Moore had before correctly remarked "are clearly inartificial,"—operating on their exposed surfaces, has formed rock-basins. Hence, it would be remarkable indeed, if rock-basins were not sometimes found upon logan-stones.

An instance has already been cited of the occurrence of rock-basins on other species of rock besides granite. It may be mentioned, in addition, that there are deep cavities of this description on the horizontal and on the slightly-inclined surface of the magnificent mass of schorl-rock at Roach, in Cornwall; in many of which the present writer has found grains of quartz and fragments of crystallized schorl, resulting from the action that produced them; which is a circumstance parallel to that related of the rock-basins in granite, by Dr. Macculloch. It may also be useful to remark, in further explanation of the process by which these cavities, in whatever rocks they occur, appear to have been formed, that, on the declivities of Roach rocks, where the water could not lodge, it has worn deep channels, instead of producing basins.

* The tendency of the action which has produced rock-basins, to extend itself equally in every direction, might, it is true, be counteracted, in directions perpendicular to the sides of a cuboidal block of granite, upon which basins were forming; by a varying resistance to disintegration, originating in an internal concentric structure in the rock. But this counteraction would merely have the effect of preventing the depth of the basins from being equal to their diameters; and would tend, rather, to increase, than to diminish the regularity of their form. Further remarks on this subject will be found in p. 336.

How

How far the Druids, or other sacerdotal orders among our more remote ancestors, may have appropriated these results of atmospheric action to their own purposes,—how far it may be true of the officiating Druid, that,

> " . . . . to wondering crowds
> And ignorant, with guileful hand he rocked
> The yielding Logan"—*

is a distinct question; and one into which it is unnecessary here to enter.

But the writer has found that other antiquarian friends are unwilling to resign, altogether, that notion of the origin of these excavations, which, in the hands of Dr. Borlase and his compeers, has given rise to so imposing a pageant of the ceremonies of Druidism: They are still desirous of attributing to the "Druid" the skill by which

> " . . . . the rocks
> That crest the grove-crowned hill he scooped, to hold
> The lustral waters."†

It may not be out of place, therefore, to state briefly the results of an examination, made in the autumn of the year 1825, of the rock-basins upon the tors or carns which crown the summit of Carnbrea Hill, near Redruth, in Cornwall; the granite of which is part of the same formation as that of Dartmoor.

This examination verified every part of Dr. Macculloch's statement, with the exception that the sides of the basins did not appear to be crumbly; while several minor facts were ascertained, which, though not adverted to in Dr. Macculloch's paper, are nevertheless entirely confirmatory of his opinion. Thus it was found that wherever the form of the cavity, and the direction and inclination of the surfaces of the rock, were such as to have admitted the water to remain for the longest space of time, in those situations the basins are always deeper than in others; and that where the water has escaped from one basin to another situated below it, a passage has been worn, which in some cases has nearly converted the two basins into one. In one instance, the thickness of an immense slab of granite, much resembling that which Borlase calls the "Sacrificing Stone," having on its upper surface six or seven rock-basins, has been cut through in several places; and the continuation of the process which formed the basins will eventually divide the slab into several blocks. In this manner many of the basins have been destroyed, by the process which originally produced them. The *side* of one basin has

* Carrington's "Dartmoor." † *Ibid.*

been

been completely perforated, while its *edge* has been left entire, forming an arch which extends over the aperture. Basins of every size, and of every stage of formation and destruction, may be found on almost every carn. The water, collected from the rains, in the basins on the so-called "Sacrificing Stone," as it flows from the upper into the lower cavities, is manifestly wearing for itself a course which, in process of time, will completely unite all the basins into one great irregular cavity.

These circumstances are clearly indicative of the natural process, still going on, by which these curious excavations have been produced, and by which also they will eventually be destroyed. But perhaps the most palpably-undeniable evidence that they cannot have been artificially formed, may be found in the circumstance, which Dr. Macculloch has not mentioned, that many of the rock-basins on Carnbrea are crossed by the veins of porphyry and porphyritic granite which traverse the carns; and which, offering a much greater resistance to the action of decomposing agents than the granite itself, have been left in the basins, in the form of ridges, their edges only having been rounded by the action of the elements. This fact is obviously conclusive; since the Druidical sculptors, who must have possessed the skill required to render some of the basins accurately spheroidal, if they were indeed the artists of them; would not have left these unsightly ridges in other basins *.

The "indications of design," stated to be afforded by "the peculiar form" of these cavities, are all, when strictly examined, evidences that they were produced by the natural process which has been described by Dr. Macculloch, and illustrated in the preceding remarks. This is also the case with every particular among those which have been so elaborately

* It may be added, in further confirmation of the natural process here maintained to have exposed these veins, that similar ridges of porphyry, several inches in height, project from the smoothed surfaces of many of the carns where there are not any rock-basins, having manifestly been denuded by the same process of weathering; and that, in some positions, the granite decomposes in a direction perpendicular to the planes of the veins which intersect it, when they become exposed, in such a manner, as to form the face of the rock.

The writer purposely refrains, in this place, from designating, more particularly than as above, the material of these veins; (although he is fully aware of the importance and interest of the subject, in the present state of our knowledge and of geological speculation, on the relations of granite to the incumbent rocks, and the origin of elvan dykes, &c.;) because it is not described in the notes from which the observations on rock-basins have been derived, and the specimens collected at the time of making them are not at present accessible to him; his memory alone, furnishing merely a general impression of their nature.

rately described by Borlase, as evincing the Druidical origin and application of these basins; and the fallacy of some of which has been acutely pointed out by the author of "Philosophy in Sport made Science in Earnest," in the additional notes to that ingenious work, vol. iii. p. 170 *et seq.*

Nothing remains to be said, in this place, on the alleged Druidical origin of rock-basins; but the writer is unwilling to quit so interesting a subject, regarding it in a scientific point of view, without adverting to some inferences connected with it, which have recently occurred to him, drawn partly from the researches of Dr. Macculloch and other geologists, and partly from his own very limited observations; respecting the internal concretionary structure ascribed to the granite of this formation.

Dr. Macculloch has attributed the "even and rounded concavity" of the rock-basins, to the "uniform texture of the granite;" and agreeably to this opinion, it has been attributed, in the foregoing remarks on Mr. Moore's objections to the equality of action permitted by this uniformity; as being, perhaps, the more strictly accurate mode of stating the fact. But may not this figure, which, in some instances, Dr. Macculloch has remarked, is as "regularly spheroidal internally as if they [the basins] had been shaped by a turning lathe," be, in reality, principally owing to that concealed spherical or rather spheroidal structure, the existence of which, in the same granite, has been rendered so highly probable by that eminent geologist? For it appears to be obvious that the same variation, in some given ratio from the centre, of the resistance to disintegration of a mass of granite, which has led to the separation of the granite of this formation into prismatic and cuboidal blocks, will, when one surface only of the rock is acted upon, and that only at certain points, give rise to cavities having a figure which is precisely that of the rock-basins. And indeed it would appear that some further cause than the equality of action permitted by the uniformity in texture of the granite, must in reality operate in the formation of these regular spheroidal basins; for if that only were the cause, the granite should be as much acted upon in a direction perpendicular to its surface, as in those directions which are parallel to it; so that the depth of the basins ought always to be equal to their diameters, or nearly so; which, so far as the writer's knowledge extends, is seldom, if ever, the case. The occurrence of the rock-basins on the vertical faces of the granite at Scilly, would seem to be a further corroboration of this idea; for it is difficult to conceive how the action of water could produce such cavities in this situation, unless it were aided by the tendency of the

rock

rock to disintegrate more easily in certain directions, with respect to the planes of its surfaces, than in others.

The form and appearance of the rock-basins on the horizontal summits and ledges of Roach rocks, and the deep channels on their highly-inclined surfaces, may perhaps be mentioned as affording some degree of support to the foregoing conjecture. Here the basins are very deep, and their outline is comparatively irregular; while the channels which the water has worn on the declivities, do not, so far as has yet been described, occur upon granite, under the same circumstances. Now, supposing the form of the basins, as they exist in granite itself, to be connected with the concentric-spheroidal structure of the rock, the facts just mentioned are what we might have expected to find, in a mass of schorl-rock like that of Roach. For although a process of disintegration has undoubtedly taken place with it, and is still proceeding, it tends to produce rather angular and pyramidal, than cubic and spheroidal blocks. From observation, therefore, we appear to have no evidence of the spheroidal structure of this particular mass of schorl-rock; and this negative result may be confirmed from theory; since the felspar, on the predominance of which, in granite, all the phænomena attendant upon and immediately succeeding its presumed original state of igneous fluidity, must have been greatly dependent, is almost entirely wanting in this rock, which is unusually uniform in constitution, consisting merely of quartz and crystals of schorl, intersected by a few veins of the former mineral *.

Another

* It is referrible, with little exception, to the first variety of schorl-rock described by Professor Sedgwick, in his memoir on the formations associated with the primitive ridge of Devonshire and Cornwall; viz. "granular quartz-rock, with deeply striated prismatic crystals of schorl, of a coal-black colour, and without regular terminations, uniformly disseminated through the mass." Trans. of Camb. Phil. Soc. vol. i. p. 106. The summit of the eastern rock, only, at Roach, consists of the fourth, or porphyritic variety, described by Prof. Sedgwick, from which, however, the imbedded crystals of felspar have disappeared, leaving the rock in a state resembling a scoria.

If the concretionary spheroidal structure, in granite, shall be found, hereafter, to have resulted from the continued action of heat upon the rock subsequently to its consolidation, as Dr. Macculloch has shown to have probably been the case with the corresponding structure in the trap-rocks, and almost demonstrated to have been so with the columnar iron-stone of Arran, the prismatic sandstone of Rum, and the columnar sandstone of Dunbar, then the inference, in the text, grounded on the presence of felspar in granite, and its absence in the schorl-rock of Roach, will become nugatory. For although the predominance of felspar, (or that of its constituents,) would certainly have exercised an important influence, in the phænomena of *crystallisation*, which must have taken place at the consolidation of the rock;

Another circumstance in the history of rock-basins appears deserving of further inquiry. Although it is very true that rock-basins do occur on the highly-inclined and even vertical surfaces of granite, in some places, yet by far the greater number of them, in Cornwall and Devonshire at least, are found either on horizontal or on gently-inclined surfaces. Has this fact any connection, it may be asked, with the structure of the rock? and does it indicate any determinate relation between

rock; we have no reason to think that its presence would materially influence the production of the *concretionary* spheroidal structure, by the means just described; since a very perfect concentric structure, radically spheroidal, has been produced, in all probability by this species of action, at Dunbar, in a rock quite devoid of felspar.

It will be perceived that the foregoing qualification of the text has been suggested, by a view of the origin of the structural phænomena presented by granite, somewhat different from that which is derivable from the inferences of Dr. Macculloch, as given in his original paper on the subject. The train of argument employed in that paper ("On the Granite Tors,"&c.) leads to the conclusion, that the interior spheroidal structure of granite resulted from the process, of crystallization, by which the fluid, consisting of the ultimate elements of that rock, in a state of igneous fusion, became solid; and in which, also, those elements were so associated together, as to form the minerals of which granite is an aggregate.

The facts and arguments, however, which Dr. Macculloch has subsequently brought forward, in his papers "On the Concretionary and Crystalline Structures of Rocks," and "On a Prismatic Structure in Sandstone induced by artificial Heat," &c., some of which have already been mentioned in this note, point to a modification of the conclusion just stated; although Dr. M. has refrained, in the latter paper at least, from extending his inferences beyond the formation of prismatic trap, and has not connected the subject, theoretically, with granite.

From these facts and arguments, then, we should be disposed to infer, that, while the *crystalline-granular* structure of granite—the crystallization of its constituent minerals, their interference with each other's forms, and the variety of relative proportions in which they are mingled,—has resulted from the crystallization of the fluid while slowly cooling—the *internal concentric-spheroidal* structure, indifferently affecting all those minerals, is a merely concretionary one resulting from the continued action of heat upon the rock, subsequently to its consolidation.

In concluding, for the present, this part of the subject, candour, however, requires a cautionary remark. Dr. Macculloch, in the two papers last mentioned, has only once alluded to granite in connection with it, when hinting, at the end of his account of the concretionary and crystalline structures, that "the granitic laminæ of the Alps.... have been produced by a concretionary arrangement analogous [only] to crystallization." The present writer, therefore, is alone accountable for the foregoing deduction from the facts adduced by that gentleman; and if there be any error in the extension of these views to the formation of granite, it is his only: while if that deduction shall be found to throw any new light upon the, at present, obscure history of that rock, the credit of it will be due, entirely, to Dr. Macculloch, of whose arguments it is a mere application; such as the mind of every geological student, it is probable, would be disposed, spontaneously, to make, after perusing the three memoirs which have been quoted.

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the direction of the axes of the spheroidal structure and any of the surfaces of the beds composing the masses of granite? Dr. Macculloch observes, (Classification of Rocks, p. 227,) that "the great laminæ, or beds of granite, are often vertical, as well as horizontal or inclined; and it thus presents continuous smooth precipices laterally, while, above, it terminates in sharp peaks." Now the beds of granite in Cornwall and Devon are horizontal, or inclined; and we find the rock-basins on their upper surfaces, which are parallel to the direction of the beds:—are the granite beds of Scilly, presenting basins on their perpendicular faces, placed in a vertical position * ?

If we view these rocks on the grand scale, and consider the schorl-rock merely as a local modification of granite, which, in a strictly geological sense, is undoubtedly the truth, we may infer that the appearances at Roach,—which have been regarded, in a former paragraph, as indicating the absence of the spheroidal structure in that mass of rock,—are merely the consequences of the vertical position of its constituent beds; and the general aspect of these rocks gives indeed some weight to this supposition, agreeing as it does with the character, described, in the foregoing quotation, as resulting from that disposition of the beds. But in this case, the conjecture that the form of the rock-basins in true granite is connected with the spheroidal structure of that rock, will perhaps derive still further support, from the difference of form in the rock-basins at Roach, which must, under this view, be regarded as excavated in the summits of the vertical laminæ; while the channels have been worn in their edges and sides. Still further would it seem, from this view, that the position and nature of the rock-basins, is connected, in some manner, with the direction of the axes of the spheroidal structure, existing in the rock.

In relation to this part of the subject, it may be well to make a remark, on the occurrence of rock-basins in the millstone grit of Ashover, mentioned near the beginning of this paper. If the conjecture, that the form, and the topical situation, of these cavities, are connected with the internal structure of the rocks on which they occur, be correct, some other indications of this structure, either in the obvious external configuration of the rock, or in the mode of its decomposition, may

* One obvious reason why rock-basins should be more frequent on horizontal than on vertical or highly-inclined surfaces, is the greater aptitude of horizontal surfaces to retain water; but the form and disposition of the basins seem to indicate a further and structural cause for this fact.

be expected to be found in the millstone-grit of this particular spot. The immediate vicinity of trap, points out a *cause* for the existence in it of such a structure; reasoning on the analogy of the case with those of the sandstones of Rum and Dunbar, as described by Dr. Macculloch*. It may also be observed, in accordance with the phænomena exhibited in those localities, that the same denudation, which has exposed, at Ashover, the millstone-grit and the shale, with the subjacent limestone and toadstone, may have removed masses of the rock last named, which may have been in contact with portions of the millstone-grit, now also removed, and thus have imparted, through their medium, a concretionary structure, to the masses of grit which still remain; and with which they were once continuous. The writer, however, is unacquainted with the particular features and history of this denudation; nor are the authorities on the subject, at present, accessible to him; he merely throws out these suggestions, therefore, for the consideration of those geologists, who may have investigated the phænomena presented by the valley of Ashover, or who can refer to the authorities alluded to.

Since the publication of the paper On the Granite Tors of Cornwall, in which Dr. Macculloch first developed his views respecting the interior spheroidal structure of granite, as indicated by the manner of its decomposition, he has pursued the subject much further, in the course of his admirable and profound researches into the history of our unstratified and crystalline rocks. He has ascertained some important limitations to the validity of the inferences, which geologists had been disposed to draw, from the desquamation, in concentric crusts, of masses of granite and of trap. He has shown, that,—while the mode of decomposition in question is in many cases indubitably the result of an internal concretionary structure in the decomposing rock,—in others it is utterly independent on any structure; being produced, solely, by the action of the atmosphere, by means, however, hitherto inexplicable; and that, in other instances, as in those, for example, from which the spheroidal structure of granite had principally been inferred, it is impossible to discover, from which of these causes the process of desquamation in reality results †. Since, however, in many cases, as just mentioned, the dependence of the process on the interior structure in question, is

* See his paper "On a Prismatic Structure in Sandstone induced by artificial Heat," &c., Quart. Journ. of Science, N.S. No. xii. pp. 263, 264.

† See Dr. Macculloch's paper "On the Desquamation of certain Rocks," &c., Quart. Journ. of Science, vol xiii. p. 237.

manifest,

manifest, and since it is impossible, in those from which the inferences had at first been drawn, to decide which of the two causes of desquamation has operated, Dr. Macculloch's original deductions appear to be unshaken by this subsequent discovery.

It is to be observed that no facts have been adduced, which prove the *absence* of the spheroidal structure, in those masses of rock, whether granite or trap, which desquamate in a manner evidently unconnected with structure, and depending only on some unknown effect of the weather. In pursuing the inquiry, therefore, it will be interesting to ascertain whether rock-basins are formed in such masses of granite, or not. If, however, the opinion expressed in this paper, that the phænomena of rock-basins are connected with a concentric structure in the rocks on which they occur, be well founded, it will afford the means of determining, whether, in those cuboidal or prismatic blocks of granite, in which desquamation takes place "all round the surface, respecting some imaginary point or centre, and promising, in the progress of time, to reduce the whole to a smaller and more spheroidal mass*," to which of the two assignable causes the process must be referred. Thus, since the granite of Cornwall and Devonshire is characterized by this mode of decomposition, while it is also abundant in rock-basins, we should infer, that the concentric desquamation it undergoes, is really dependent on the internal concretionary structure to which it had originally been referred.

To conclude, for the present, these remarks on the geological history of rock-basins:—Mr. De la Beche, in the explanation of his "Sections and Views illustrative of Geological Phænomena," lately published, after quoting Dr. Macculloch's paper On the Granite Tors, as explaining the Views taken from it of the Vixen Tor, the Cheesewring, and the Logan at Castle Trereen, observes, "Looking at these drawings, and taking into consideration the comparatively little waste which the objects they represent now suffer, it would seem to require a long lapse of time to produce the effects we here witness." In this opinion every one must agree; but the present writer would suggest, from a few observations of his own, that more rapid, though far less extensive changes, are effected by that particular species of action, which has produced, and is now producing, as well as destroying, rock-basins; and the effect of which, in dividing masses of granite, and changing their figure, is well exhibited in the Tors of Carnbrea.

* Dr. Macculloch's paper last quoted, p. 239.

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The whole of the foregoing remarks are submitted, with much deference, to the consideration of geologists; as supplementary to Dr. Macculloch's memoirs on the concretionary structure in rocks, and as hints, for future investigation, by those who have it in their power to institute extensive researches in Geology.

Hazelwood School, near Birmingham,
Oct. 10th, 1830.

LI. *On the Discharge of a Jet of Water under Water. By* R. W. Fox, *Esq.*

To the Editors of the Philosophical Magazine and Annals.

I AM not aware that it has been before noticed, that a jet of water discharges the same quantity, in water, as in air, in a given time, without reference to the depth or the motion of the water; at least within certain limits.

Thus when the experiment was tried with a head of water six feet high, the same orifice discharged equal quantities in equal times in air, in still water, and in a rapid stream, moving at the rate of about six feet in a second; the jet having in one case been turned with the current, and in another against it: and when by lengthening the tube, the aperture was submerged to the depth of fifteen feet, the effect was the same as at the surface, under the pressure of an equal column above it.

These results have been obtained by my brother Alfred Fox, and myself;—and you may, perhaps, think them deserving a place in your Magazine, if they should appear to you to be new.

We sometimes coloured the water, when the jet appeared to pass unbroken to a considerable distance under the water.

Falmouth, 10th month, 9th, 1830. R. W. Fox.

LII. *Additions to the Theory of Eclipses, and the Methods of calculating their Results. By Professor* Bessel.

[Continued from page 275.]

[7.] THE calculation of an occultation of a star can now be performed, after the preparatory operations explained in the preceding section, in two different ways. The first supposes that the same value of T is to be applied to all observations which are to be calculated, in which case p and q correspond to the value a in the arrangement above given. On this

this supposition the values of P and Q corresponding to the time T + T', are obtained by this formula:

$$(7) \ldots a + T'.b + \frac{T'^2}{2}.c + \frac{T'.T'^2-1}{2.3}.d + \frac{T'^2.T'^2-1}{2.3.4}e + \&c.$$

consequently, p' and q' by this formula:

$$(8) \ldots b + \frac{T'}{2}c + \frac{T'^2-1}{2.3}d + \frac{T'.T'^2-1}{2.3.4} + \&c.$$

by which N and log n are found. If, however, several observations are to be calculated at the same time, it is more convenient at once to calculate the values of N and log n for different values of T'; they are obtained by putting for n sin N and n cos N these expressions:

For $T = -2$ .... $b - c + \frac{1}{2}d - \frac{1}{4}e$
$= -1$ .... $b - \frac{1}{2}c$
$= \quad 0$ .... $b - \frac{1}{6}d$
$= +1$ .... $b + \frac{1}{2}c$
$= +2$ .... $b + c + \frac{1}{2}d + \frac{1}{4}e.$

For every single observation M and log m are then to be calculated by the formulæ

$$m . \sin M = p - u, \qquad m \cos M = q - v$$

in which p and q are the known values of P and Q for the time T; at last the sixth formula is to be calculated. A second approximation is not necessary if the value of $T' = t - d - T$, or the assumed difference of meridians employed in the first is correct within some minutes of time, which may always be supposed. Such an approximation would, however, cause little trouble, as the calculation of formula (6), only, would be affected by the alterations. A second way of performing the calculation would be to calculate by interpolation (7) from the values of P and Q corresponding to T, T $\mp$ 1, &c. those values of these quantities which belong to the time of observation, reduced to the first meridian by an assumed difference of meridians, and to use these values instead of p and q and their differential quotients, instead of p' and q'. The latter are found by the formula

$$(9) \ldots b + T'.c + \frac{3T'^2-1}{2.3}d + \frac{2T'^3-T'}{3.4}e + \&c.$$

by the application of which, consequently, N and log n will be obtained. If it should be preferred at once to find N and log n, and to interpolate between the values thus calculated, they might be obtained by the expression for n sin N and n cos N,

For

$$\begin{aligned}
\text{For } T' &= -2 \ldots\ldots b - 2c + \tfrac{11}{6}d - \tfrac{7}{6}e \\
&= -1 \ldots\ldots b - c + \tfrac{1}{3}d - \tfrac{1}{12}e \\
&= 0 \ldots\ldots b - \tfrac{1}{6}d \\
&= +1 \ldots\ldots b + c + \tfrac{1}{3}d + \tfrac{1}{12}e \\
&= +2 \ldots\ldots b + 2c + \tfrac{11}{6}d + \tfrac{7}{6}e
\end{aligned}$$

The remainder of the calculation is as above.

This second manner of conducting the calculation, supposes, therefore, the determination of p and q by interpolation, while the former one constantly proceeds from the same values of these quantities. But it has the advantage of a more easy calculation of the term (6) $\frac{m}{n} \frac{\cos(M-N-\psi)}{\cos\psi}$ which is the correction of the assumed difference of meridians, and which is, of course, commonly very small; its convergency to the truth is likewise the greatest possible, and the error of the first approximation arises only from the moon's motion during the interval between the supposed and the real difference of meridians being taken as it would be at the beginning, or at the end of this interval of time, while it ought to have been taken for the middle of it. For the observatories of Europe, whose meridians are very nearly known, the square of $n\,(T'+i)$ might even be neglected, and the equation to be resolved might thus be reduced to one of the first degree. But all these advantages of the second method of calculating appear to me to be insignificant in comparison to the trouble of the interpolation for finding p and q. I therefore prefer the first. In the application of these formulæ, however, the interpolation will never require to be carried beyond the second differences, and consequently three values of P and Q will be sufficient.

[8.] The method here explained leaves it to the choice of the calculator, whether the quantities α, δ, shall refer to the equator or to the ecliptic. In the result of the calculation there is nothing referring to either of these great circles, and they serve only to denote the relative situation of the various points of the celestial sphere referred to in the problem. The former is, however, always more easy, if the places of the moon in relation to the equator are contained in an ephemeris; and even if this should not be the case, whenever several observations are to be calculated at the same time. But when there are few observations, and when the places of the moon are to be derived from the tables themselves, or are to be be taken from an ephemeris containing the moon's longitude and latitude, the preparatory calculations required in finding the quantities

quantities referring to the equator become more troublesome than the calculation of the longitude and latitude of the zenith: in that case the ecliptic deserves the preference. The tables give the longitude, latitude, and parallax of the moon for the time T, and likewise their variations for the preceding and following hours. These three longitudes and latitudes are to be converted into right ascension and declination, if the equator is to be used. This trouble is saved if we calculate with longitude and latitude; but, on the contrary, in using right ascension and declination we dispense with the calculation of the longitude and latitude of the star and the zenith. If, therefore, in the case that the places of the moon in relation to the equator are unknown, only two or three observations are to be calculated, the ecliptic appears to deserve the preference. If a single observation is to be compared with the ephemerides or the tables, it is more advantageous to calculate p' and q' from the hourly variations of α, δ, π, than to derive them from the three values of P and Q. In this case it will be most convenient to assume for T, the time of the observation reduced to the first meridian, by applying an approximate value of the difference of meridians, and to calculate for this moment P and Q, as also their differential quotients p' and q', by the formulæ

$$(10)\begin{cases} p' = \left(\frac{\cos\delta\cos(\alpha - A)}{\omega\cos\pi}\cdot\frac{d\alpha}{dt} - \frac{\sin\delta\sin(\alpha - A)}{\omega\sin\pi}\cdot\frac{d\delta}{dt} - \frac{p}{\omega\,\mathrm{tang}\,\pi}\cdot\frac{d\pi}{dt}\right. \\ q' = \left(\frac{\cos\delta\sin D\sin(\alpha - A)}{\omega\sin\pi}\cdot\frac{d\alpha}{dt} + \frac{\cos\delta\cos D + \sin\delta\sin D\cos(\alpha - A)}{\omega\sin\pi}\cdot\frac{d\delta}{dt}\right. \\ \qquad\qquad - \left.\frac{q}{\omega\,\mathrm{tang}\,\pi}\cdot\frac{d\pi}{dt}\right) \end{cases}$$

in which $\frac{d\alpha}{dt}$, $\frac{d\delta}{dt}$, $\frac{d\pi}{dt}$ signify the hourly motions, and ω the radius of the circle expressed in seconds.

[9.] It now remains further to develope that part of formula (6) which is dependent on the corrections of the elements of the calculation. The result obtained by the method here explained, is not so much the difference of the meridian of the place of observation, as the relation between it and the elements used in the calculation, and by the combination of several observations one or more of the elements of the calculation are eliminated, and the result is thus made partly or entirely independent of the tables.

In section [5] the quantities i and i' have been so assumed as to give these equations:

$$p'i - q'.i' = a\Delta\alpha + b\Delta\delta + c\Delta\pi + d\Delta e^{s}$$
$$q'i + p'i' = a'\Delta\alpha + b'\Delta\delta + c'\Delta\pi + d'\Delta e^{s}$$

$\Delta\alpha$, $\Delta\delta$, &c. are here assumed as expressed in parts of the radius; they must, therefore, be divided by $\omega = 206265$ if they are meant to be given in seconds. The coefficients $a, b \ldots a' b'$ are the differential quotients of $P - u$, and $Q - v$ in relation to α, δ, π and e^2; neglecting in their values the small quantities of the order of $\alpha - A$ and $\delta - D$, which, on account of the smallness of $\Delta\alpha, \Delta\delta$, &c. will produce no error of any consequence, the expressions for them will become very simple: $a = \frac{\cos\delta}{\sin\pi}$; $a' = 0$; $b = 0$; $b' = \frac{1}{\sin\pi}$; $c = -\frac{P}{\tan\pi}$; $c' = -\frac{Q}{\tan\pi}$; $d = -\frac{du}{d.e^2}$; $d' = -\frac{dv}{d.e^2}$. Adopting these expressions, and substituting $n \sin N$ and $n \cos N$ for p' and q', and h for $\frac{s}{\omega . n . \sin\pi}$, where s represents the number of seconds equal to the hour to which p' and q' belong [6], we obtain

$$i = h \sin N . \cos\delta\,\Delta\alpha + h\cos N\,\Delta\delta - h\cos\pi . \Delta\pi\,\{P\sin N + Q\cos N] - h\,\omega\sin\pi . \Delta e^2\left[\frac{du}{d.e^2}\sin N + \frac{dv}{d.e^2}\cos N\right]$$

$$i' = -h\cos N . \cos\delta . \Delta\alpha + h\sin N . \Delta\delta + h\cos\pi . \Delta\pi\,[P\cos N - Q\sin N] + h.\omega\sin\pi . \Delta . e^2\left[\frac{du}{d.e^2}\cos N - \frac{dv}{d.e^2}\sin N\right].$$ Hence,

$$\left(i + \frac{i'}{\tan\psi}\right)\frac{\sin\psi}{h} = -\cos(N+\psi).\cos\delta.\Delta\alpha + \sin(N+\psi)\Delta\delta + \cos\pi . \Delta\pi\,[P\cos(N+\psi) - Q\sin(N+\psi)] + \omega\sin\pi\,\Delta e^2 \times \left[\frac{du}{d.e^2}\cos(N+\psi) - \frac{dv}{d.e^2}\sin(N+\psi)\right].$$

The part dependent on $\Delta\pi$ may be reduced to a more convenient form, and the one dependent on $\Delta . e^2$ may be further developed. Substituting in the former $p + n\sin N . T'$ and $q + n\cos N . T'$ for P and Q, it becomes $\cos\pi . \Delta\pi\,[p\cos(N+\psi) - q\sin(N+\psi) - n . T'\sin\psi] = \cos\pi . \Delta\pi\,[(p\cos N - q\sin N)\cos\psi - (p\sin N + q\cos N + n\,T')\sin\psi]$; and further, making $x = q\sin N - p\cos N$, $n\tau = n\,T - p\sin N - q\cos N$, and putting for T' its expression $\frac{t-d-T}{s}$, it assumes this form

$$-\cos\pi . \Delta\pi\left[x\cos\psi + \frac{n}{s}(t - d - \tau)\sin\psi\right]$$

It will be easily perceived that $x\,\omega\sin\pi$ is the smallest distance of the true path of the moon from the star, positive if the moon passes to the north, negative if she passes to the south

south of it, and τ the time of the nearest conjunction counted from the first meridian.

The development of the influence of $\Delta . e^2$ depends on the differential quotients in relation to e^2 of the quantities $r \cos \phi'$ and $r \sin \phi'$ which occur in the expression for v and u. But these quantities are differently expressed by e^2 and the observed latitude ϕ, according as ϕ' denotes the declination or the latitude of the zenith. The formulæ to be employed in the two cases must, therefore, be separately developed, while all the preceding ones equally apply to both cases. I begin with the case of ϕ' denoting the declination.

We have in this case, $r \cos \phi' = \frac{\cos \phi}{\sqrt{(1-e^2 \sin \phi^2)}}$; $r \sin \phi' = \frac{(1-e^2) \sin \phi}{\sqrt{(1-e^2 \sin \phi^2)}}$ whence $\frac{d . r \cos \phi'}{d . e^2} = r \cos \phi' . \frac{r^2 \sin \phi'^2}{2(1-e^2)^2}$; $\frac{d\, r \sin \phi'}{d . e^2} = r \sin \phi' . \frac{r^2 \sin \phi'^2}{2(1-e^2)^2} - \frac{r \sin \phi'}{1-e^2}$ or putting $\beta = \frac{r \sin \phi'}{1-e^2}$,

$\frac{d . r \cos \phi'}{d . e^2} = \frac{1}{2} \beta^2\, r \cos \phi'$; $\frac{d . r \sin \phi'}{d . e^2} = \frac{1}{2} \beta^2 . r \sin \phi' - \beta$.

The expression for u and v being these:

$$u = r \cos \phi' . \sin (\mu - A); \; v = r \sin \phi' \cos D - r \cos \phi' \sin D \cos (\mu - A),$$

we obtain $\frac{d u}{d . e^2} = \frac{1}{2}\beta^2 . u$ and $\frac{d v}{d . e^2} = \frac{1}{2} \beta^2 . v - \beta \cos D$;

and, next, the part dependent on $\Delta e^2 = \omega \sin \pi . \Delta e^2 \times$

$$\left[\frac{1}{2} \beta^2 \Big(u \cos (N + \psi) - v \sin (N + \psi)\Big) + \beta \cos D \sin (N + \psi)\right]$$

That part of this expression which is multiplied into $\frac{1}{2} \beta^2$ may be written: $P \cos (N + \psi) - Q \sin (N + \psi) - (P - u) \times \cos (N + \psi) + (Q - v) \sin (N + \psi)$; and we may substitute for $P - u$ and $Q - v$ their equivalents $m \sin M + n \sin N . T'$, and $m \cos M + n \cos N . T'$ by which we have $(P - u) \cos (N + \psi) - (Q - v) \times \sin (N + \psi) = m . \sin (M - N - \psi) - n T' \sin \psi =$ (substituting for T' its value [5] —

$$\frac{m . (\cos M - N - \psi)}{n \cos \psi}\Big) - \frac{. m \sin (M - N)}{\cos \psi} = k.$$

If we here apply the above transformation of $P \cos (N + \psi) - Q \sin (N + \psi)$ the expression dependent on $\Delta . e e$ will become: =

$$- \omega \sin \pi . \Delta e^2 \left[\frac{1}{2} \beta^2 \left[x \cos \psi + \frac{n}{s} (t - d - \tau) \sin \psi - k\right] - \beta \cos D \sin (N + \psi)\right].$$

[To be continued.]

LIII. *Notes on the New Red Sandstone of the County of Durham, below the Magnesian Limestone.* *By* WM. HUTTON, Esq. *F.G.S.**

PROF. SEDGWICK, in an elaborate paper just published in the Transactions of the Geological Society, has been the first to point out the true relations of a bed of sandstone, found underlying the magnesian limestone and conformable to it, but unconformable to the coal measures.

This sandstone had before been observed by Smith, who figures it in his Geological Map of Yorkshire (published in 1821), as the Pontefract rock; but he mistakes its true nature and value, and classes it with the coal sandstones. Professor Sedgwick corrects this error, and very satisfactorily, upon a general view of the whole formation, from Nottingham to the banks of the Tyne, proves it to be a bed of new red sandstone subordinate to the magnesian limestone, and also points out its analogy with the "rothe-todte-liegende" of the German geologists; thus adding another link to the chain connecting the formations of this country with those of the continent, and one which is of the more importance, as it serves to clear up many doubts and difficulties which have hitherto existed, in the comparison of our strata with those of Germany.

In treating of the situation of rocks on a district of great extent, it is absolutely necessary to consider the formations or usual groupings of certain beds, under their general and more prominent characters:—in this way, of course, Professor Sedgwick has considered the strata under review, and has, in a very clear and comprehensive manner, pointed out their general relations; but when we examine the geology of a small district, the different beds composing the formations cannot be marked too minutely; and this more especially, when these beds possess any local interest, or œconomical value. The sandstone beneath the magnesian limestone, besides being a member of our strata, with which we were not before acquainted, is one of considerable importance to all the owners of property where it exists, particularly to all those who may have any intention of working coal beneath it. Considering it in this view to deserve our attention, I beg to lay before the Society the following details of an examination of its edge, throughout the whole of the county of Durham, and the ideas that have suggested themselves by the survey; and this I do with the greater confidence, having had the advantage of the

* Read before the Natural History Society of Northumberland, Durham, and Newcastle-upon-Tyne, April 20, 1830, and published in their Transactions.

skill

skill and professional experience of my friend, Mr. Francis Forster, along the whole of the line.

The beds composing the lower new red sandstone vary considerably both in mineral character and thickness: one great division may, however, be satisfactorily established, viz. an upper and a lower bed.

The upper bed is generally a running sand, occasionally it may be considered as a sandstone, but it never possesses coherence enough to be of use as a building material; it has interspersed through it, rounded grains of white quartz, which occur very irregularly, but are often arranged in lines parallel to the planes of stratification; the prevailing colour is a very light buff. Between this and the lower bed, which is more consolidated, and of a red colour, the division is generally well marked, but sometimes the two pass into each other insensibly, the sand gradually changing its hue, becoming compact and micaceous. The colour of the lower stratum is a character which varies exceedingly; it is at all times, more or less, red, sometimes purple; but almost every quarry furnishes beds, which, when taken alone, have little in their aspect to distinguish them from a common coal grit; sometimes it is light yellow, or nearly white, with zones or bands of deep red, and frequently the colouring matter is in veins and spots. The characteristic colour of this rock appears to arise from oxide of iron disseminated through it, but this is frequently united with a clayey matter, forming nodules of "ruddle," which are irregularly embedded in the stone. The state in which iron exists in this formation is different from that of the coal measures generally;—in no instance were we able to discover a nodule of clay ironstone, in the different beds composing it. The texture of the lower bed also varies considerably, it being sometimes of a fine even grain, and compact enough to be worked as a building stone; at other times it is coarse and uneven, from the quantity and size of the embedded grains and nodules of quartz. It is always micaceous, and some of its beds so much so, as to split into thin laminæ of a bright red colour, which are very prone to decomposition.

A conspicuous character of this lower stratum is, the false bedding of the stone, which may be seen in every quarry; but, perhaps, the best character of all to distinguish it from a coal grit, is the total absence of vegetable organic remains.

This sandstone, in the course of its outcrop, follows generally that of the magnesian limestone, but occasionally projects beyond, where it is hard and of considerable thickness.

For the sake of perspicuity in the following remarks, we shall

shall designate the upper bed as the Yellow Sand, from its prevailing character, and the lower as the Red Sandstone; but without in the least wishing them to be considered in any other light than as different members of the same formation.

When the magnesian limestone first enters the county of Durham from the south, its course is so little marked for several miles, from the lowness of the level at which it runs, that the beds beneath it cannot easily be examined:—proceeding northward, however, it begins to rise into those round-topped hills which form the general character of its edge throughout the county; the red sandstone keeps its course near the foot of these hills, and was first met with on the side of the road leading from Legs Cross Toll Bar, towards Heighington; about a hundred yards beyond, the well-known Cockfield Dyke crosses the road; the basalt, which has been worked for a road-stone, is seen cutting through the yellow sand.

Park House Quarry, on the hill side, one mile west of Heighington, is worked entirely in the red sandstone; it has been lately opened out; the stone is of a close texture, and is used for gate-posts, &c.; the limestone is not seen in the quarry, but it forms the capping of the hill above, and is extensively worked about three hundred yards to the east.

At Brusselton, near the top of the hill, close by the turn of the road, the yellow sand is visible on the road side. In the great quarry beneath the tower, there is a very fine display of the red sandstone in all its variety of colour and texture, having beds or seams of a hard, light blue, siliceous shale, running through it in the most irregular manner; the bed must be here of considerable thickness, as the quarry has been worked at least fifty feet deep.

In Thickley Quarry, by the side of the Darlington railway, the same kind of stone is extensively worked, as is found at Park House Quarry and Brusselton. The upper bed is a slaty limestone; a light blueish white clay occurs here in irregular seams. It was in a quarry a little to the east of this, by the side of the railway, that the curious deposit of fossil fish occurred, which is described by Professor Sedgwick in his Memoir; they were found in a slaty limestone bed, very near the sandstone.

At Eldon, the limestone forms the top of the hill, and proceeding northward towards Howlish Hall, although the red sandstone does not appear at the surface, yet there is sufficient evidence of its existence beneath, in the deep rich red colour of the soil on the slope of the hills immediately under the limestone.

At Cowndon, a fault, running nearly north and south, throws

throws down the limestone, so as to make it abut against the coal measures.

On the slope of the hill rising towards Westerton, near a well, in the middle of a large pasture field, the red sandstone comes to the day. At the top of the hill the true relations of the rocks are not easily understood; the east end of the village is upon limestone, but there is a sandstone at the surface, in the road about one hundred yards to the westward, which is no doubt caused by a fault. This sandstone may be seen in a small quarry at the west end of the village; but whether it belongs to the red sandstone, or to the coal measures, is difficult to determine; it had large coarse grains of quartz in it, apparently rounded by attrition.

The limestone forms the top of the hill above Quarrington, below it is the yellow sand, and beneath this the red sandstone, which is here thin, and of the very micaceous variety. The upper part of the yellow sand is hard, apparently from the infiltration of calcareous matter from the limestone.

Heugh Hall Hill is principally of limestone, but near the bottom of its northern slope, the sand makes its appearance having bands of a red colour in it, marking the planes of stratification; in a field at the foot of the hill called "Red Brae Bank," the red sandstone has been lately bored through in search of coal.

On the slope of the hill above Pittington, both members of this formation may be seen cropping out beneath the limestone, but of inconsiderable thickness; the sandstone is of the micaceous variety, splitting into thin leaves.

Near a limestone quarry on the hill between Pittington and Moorsley, a thin bed of the same red micaceous shaly sandstone appears, having above, or in it, a seam of blueish-white unctuous clay. The quarry here is extensively worked in the slaty limestone; a kiln is built upon the yellow sand, which, at its upper part, has hard beds of a calcareous nature alternating with it.

The same light-coloured clay is visible on the side of the footpath leading towards Moorsley, and still further on, about half a mile short of that place, the yellow sand again appears beneath the limestone.

At Moorsley, a new pit has been sunk by Mr. Russell, which was begun upon the yellow sand, and immediately below the limestone. The sand is here about sixteen feet thick, and the red sandstone three fathoms, having a shale bed beneath it.

In sinking the old pit, at Hetton, below the yellow sand, the red sandstone was found, between three and four fathoms thick;

thick; this bed is here well known and calculated upon by the sinkers, as unconformable to the coal measures, it having been proved by borings in search of coal, in many places in the neighbourhood.

In the Downs Pit, at Eppleton, the sinking was through a dry yellow sand, about six fathoms, and the red sandstone three fathoms.

The quarry in Rough Dean, near Houghton-le-Spring, displays the slaty limestone much mixed with seams of a yellow clay, resting upon the sand, sometimes in a very uneven line, the limestone appearing to bend round and conform itself to the inequalities of the sand, which is of a light colour, having many hard veins of a calcareous nature in it, and also seams of clay. It appears about twenty feet thick; there is at its lower part a bed of white-coloured unctuous clay, precisely similar to that observed between Moorsley and Pittington, and in the quarry at Thickley. The general character and appearance of the red sandstone, which is worked here as a building material, agree exactly with that of the Thickley, Brusselton, and Park House quarries.

The old quarry at the foot of Houghton Hill, which is worked down to the sand, is, at its lower part, slaty, having thin layers of a brown clay alternating with the limestone. In a bed about five feet from the bottom of the quarry, a few impressions of fish have been lately found, very well preserved, having their scales remaining perfect; the yellow sand is here at least sixty feet thick.

The new pit, sunk by Lord Durham, at the foot of the hill, was begun upon the sand, and beneath it was a thin bed of sandstone, of a brick-red colour, of a rough grain, and having white earthy felspar disseminated through it.

At Newbottle, in consequence of a fault, the limestone is thrown down, and made to abut against the red sandstone at the south end of the village; on the hill side, proceeding northward, a quarry is just opened in the sandstone.

From this point towards Pensher Hill, although the rock is nowhere visible, the situation of the sandstone is sufficiently marked, by the belt of red ground, formed at its outcrop, which may be thus easily traced by the eye beneath the limestone *.

* The colour of the soil over the outcrop of the lower new red sandstone, which is more conspicuous in some parts than others, is a character to be observed generally; thus pointing out its situation, although the rock itself be not visible, and no doubt in this character have originated the names of several places upon the line; as Red-worth, Red-house, Red-brae-bank, &c.

Pensher

Pensher Hill is well known as the most conspicuous point upon the edge of the magnesian limestone formation; the yellow sand appears, exhibiting all its usual characters, on the northern face of the hill, about half-way down.

Clack's Heugh presents a very bold cliff of limestone, resting upon the yellow sand, here forming a bed of immense thickness; the coal measures are made to abut against the limestone by a fault which traverses the eastern end of the cliff; and a portion of the red sandstone has also been forced up, so as to form the uppermost bed. This is the only point at which I have observed it on the south bank of the Wear, although I am informed it exists higher up, near to Hylton Ferry; on the opposite side, however, it forms a most conspicuous object, being there of great thickness, and generally of a dark reddish-purple colour, the yellow sand forming its upper member. Above Burn's Quay is a quarry where it is worked for fire-stone; it is close-grained, but not very hard, and of a pretty even texture.

In the great Pallion Quarry the limestone is worked more than sixty feet thick; the lower slaty beds are of a blueish-gray colour, and being found to make good lime (probably from the absence of magnesia), they are worked entirely away: they are found to rest upon what the quarrymen call "black stone;" a tough brown shale about seven feet thick; and below this the yellow sand occurs to an unknown depth. It was in the lower beds of the limestone, in this quarry, that the fossil fish occurred, which is figured in the fourth volume of the Geological Transactions, plate II.

In the little dell running up from the Wear towards Hylton Castle, the yellow sand may be observed beneath the limestone, of considerable thickness.

At Down Hill, near West Boldon, we again have the sand; it cannot be seen in the quarry, but makes its appearance, at each end of the escarpment, beneath the slaty beds of the limestone; a single fossil fish has occurred in this quarry.

In West Boldon, limestone is worked on the hill, below the church; in the quarry the yellow sand is not visible, but it appeared in cutting the foundations of a house below, and a well not far from the gate of the rectory was begun in it; this bed must here be of inconsiderable thickness, as the red sandstone was immediately come upon, and was sunk into to the depth of twenty-nine feet; the upper part was micaceous, splitting into thin layers, but the stone became more compact as the operations were carried deeper.

In a little dell near Westoe, called the Deans, behind a house called Brinkburn House the red sandstone occurs, and

I am informed by a gentleman well acquainted with the neighbourhood, that some years ago the yellow sand was worked in a pit near to Harton Toll Bar, on the turnpike road from South Shields to Sunderland. At the mouth of the abovementioned Deans, near a brewery, a sandstone of the coal measures is visible, on the western side of the burn; rising to the top of the hill, on the opposite side, we find the new red sandstone, in Mr. Fox's Quarry, which has been extensively worked for a building stone; it may be again seen in the Colliery Quarry, on the road side leading to Westoe. The magnesian limestone, which forms the capping of the hill, being also worked in an adjoining field.

In a quarry at Lay Gate, near South Shields, the red sandstone forms the upper bed, and rests upon a white sandstone, which is evidently, from its general characters, a coal grit, having abundance of vegetable remains in it. This quarry was the last point at which the red rock was observed south of the Tyne; but on the north side it occurs again, forming the cliffs below the Spanish Battery. Here neither the limestone nor the yellow sand appear, but on the other side of Tynemouth Haven the limestone forms the uppermost bed in the Castle Cliff, and at its lower part alternates with the yellow sand; the latter is upwards of twenty feet thick, and is seen resting upon the red sandstone, which forms the whole of the lower part of the cliff on its south and eastern faces.

A well-known basaltic dyke here cuts through the red sandstone, and the yellow sand; but is not seen in contact with the limestone. In the eastern face of the cliff, several faults appear traversing and affecting alike the whole formation.

At the base of the cliff, on the northern side, in Percy Haven, a coal sandstone makes its appearance, rising rapidly towards the north-west, being surmounted by the superior formation; both members of which may be traced all round this haven, and extending to a point a little short of the Two-Gun Battery, where they are cut out by the rising of the beds of the coal formation.

[To be continued.]

LIV. *Remarks on* Mr. Babbage's *Work "On the Decline of Science in England." By* A Correspondent.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

IN looking over Mr. Babbage's late publication "On the Decline of Science in England," I regret much to see that so able a man as he is, and one who has hitherto been distinguished

guished for candour and urbanity, should have thought fit to make a severe attack on the Royal Society, to which, as a man of Science, he owes much good-will; and to cover this, which appears to be the main object of this work, by a title which communicates little of its real nature and design. The harshness of his strictures he represents as absolutely necessary to cure the deep-rooted maladies of that body; but yet the discipline employed by him, very much resembles that of an ardent and inexperienced son of Galen, who, in his eagerness to display a supposed manual dexterity, overlooks, entirely, the constitution with which he has to deal, and the mode by which a permanent cure is most likely to be effected.

It is not dignified in Mr. Babbage, to decline in his title-page, the designation of *Fellow of the Royal Society*. It is much beneath him to affect to despise a title, which his friend Wollaston invariably employed; and which it is not very likely that he would have ventured to put aside, during the life-time of that great man. This is a petitesse unworthy of Mr. Babbage:—it is only, however, a petitesse; but his charge against the Secretaries of the Royal Society, of wilfully suppressing an important document of the Council; and his fancied detection of their guilt, involve his candour, or his judgement, in a way which has excited much surprise. The circumstances of this charge have been sufficiently discussed by Dr. Roget and Mr. Babbage, in some of your former Numbers; and I would now ask, whether it was consistent with the usual courtesy of society, to bring forward a serious accusation, in the face of the world, without taking the trouble to inquire of the accused party, whether there was any ground for mistake, or misapprehension, on the subject?

In the privacy of the study, a man may come to an unimpeachable conclusion, in the exact sciences, from his own internal resources. But in the affairs of the world, and particularly in matters involving character, he must recollect, that there are two sides of a question; and that, in determining guilt, there is not only a necessity of rigidly ascertaining facts; but also the *animus* connected with them, on which alone their real tendency depends.

Mr. Babbage's criticisms on the mode of entering the minutes of public meetings may to a certain degree be in point; and would, I have no doubt, have received respectful attention, if submitted in a friendly manner; but to think of founding, as he has done, a charge of bad faith on any thing which he has been able to elicit, relative to the transaction in question; to consider himself justified in applying innuendoes of tam-

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pering with written documents, honourable men, for whose personal character he represents himself as entertaining a high value, is uncourteous, uncandid, and unjust.

Mr. Babbage is anxious for a reform in the Royal Society; so he is in the Universities: but he sets about the accomplishment of his wishes, in a way, which the least knowledge of the world might convince him, was sure to defeat his object. His efforts, like those of many other sticklers for innovation, by attempting too much, and too speedily; viz. preferring the tone of acrid and vituperative censure, to that of mild and manly expostulation, run the risk of preventing the adoption of those moderate views of improvement, in which judicious and temperate men would all be disposed to join. He seems to mis-time his motions, and appears to be grievously offended that an ancient establishment does not at once consider his suggestions for its improvement, as mandates to be obeyed, not advice to be examined. The monarchical influence of which he complains, is not of modern date, it has existed in the Society for a long series of years; but it would be well to recollect, that much may be done, in no very long period, by temperate and well-timed counsel; and that in the policy of Societies and States, as well as in that of domestic life, a little blindness to faults, and a little kindness to virtues, are among the most agreeable and efficient, as well as the best established maxims of good government.

The Society Utopist may with advantage attend to the advice of the great Bacon, when he says, "that it were good that men, in their innovations, would follow the example of time itself, which indeed innovateth greatly, but quietly, and by degrees scarce to be perceived; for otherwise whatsoever is new, is unlooked for; and ever it mends some and pairs others."

Mr. Babbage is very liberal in his application of terms of reproach to the Royal Society, which he represents as being in a "second childhood," and as being viewed "with contempt in this country, and ridicule in others." At what time its demerits arose to this fearful extent, it is difficult to conjecture; but I presume they have been of long standing, since they are traced to "years of misrule, to which it has been submitted:" and yet Mr. Babbage has, in several instances of late years, thought it not beneath him to communicate materials to the Society's Transactions; and even at a still later period, to sit down at the Board with the junto, whom he is at so much pains to expose.

The faults of the Royal Society are visited upon the present President and Secretaries, by Mr. Babbage, with nearly

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as singular a severity as those of the lamb by the wolf in the fable; and the misrule of years is blazoned, on the present occasion, just as if it could with justice be laid at their door. The Royal Society is erroneously represented as having, in turns, given "the most determined opposition" to the establishment of the Linnæan, the Geological, and the Astronomical Society. The most active supporters of each of those Societies, were, however, fellows of the Royal Society: and with regard to the first-named of them, the Linnæan, it is well known, that Sir James Smith had the most important and effectual assistance of Sir Joseph Banks, in its formation. So far, even Sir Joseph is free from any discredit on the subject. But with regard to the Geological and Astronomical Societies, though Sir Joseph personally disapproved of them, it could not fairly be said that they were discountenanced, either by the Royal Society, or its Council.—Dr. Wollaston was an early member of the Geological Society, though one of the most constant, efficient, and distinguished members of the Royal Society's Council; and the present President, as is well known, has long given it his hearty support; and not a great while ago successfully employed his influence with Sir Robert Peel, to transfer the present apartments of the Geological Society, from the Royal Society, to that body. Mr. Davies Gilbert was likewise an early and active member of the Astronomical Society; so were Mr. Pond, the astronomer-royal; Sir Wm. Herschel; Mr. Herschel his highly-gifted son; Dr. Wollaston; Mr. South; Mr. Groombridge; Dr. Roget the present Secretary, and many others, who, like those just mentioned, from being more or less frequently on the Council of the Royal Society, were, according to Mr. Babbage, more or less worthy of being held up to public odium, as having submitted to be under the undue influence of the President.

The habits of obedience and command, which attach to the military character, are adduced as reasons why Captain Sabine could not be expected to act with "the perfect freedom which should reign in the Councils of Science:" and yet the ardour with which it appears that men of Science are embued for "loaves and fishes;" the patronage represented to be exercised to "staunch supporters of the system;" the disposition which we learn has been so generally exhibited in the Council, to divide "all the good things" among the members of their coterie,—required not, surely, the introduction of a military officer into that body, to exhibit an example of implicit obedience.

It will hardly be gratifying to the world of Science, to have it held out to them, that Dr. Wollaston, Sir Humphry Davy, and

and Dr. Young, among the dead; and Mr. Herschel among the living, not only condescended to be the humble and obedient creatures of a president of the Royal Society, as members of the Council; but to receive, as its Secretaries, the wages appropriated generally to his most staunch supporters; and I cannot myself but regard it as somewhat mortifying, that a gentleman, a man of education, and one who deservedly enjoys the high dignity of filling a chair once occupied by Newton, should have brought forward imputations, which seem necessarily to lead to those conclusions.

That there are many things which require change in the management of the affairs of the Royal Society, I am quite willing to admit; and harsh as has been the conduct exercised by Mr. Babbage towards that body, I trust that his plan of improvement, and the various other topics embraced by him, will be considered with candour.

One of the first measures requiring alteration, is relative to the house list. That this list should be determined on by the retiring Council, as was the case on the last occasion, is highly expedient; and a president need not fear an undue interference with his prerogative, or entertain any apprehensions that his real and substantial weight, either with the Society at large, or the Council, will be diminished, by dividing the responsibility of such list, with the other members of the Council. The Society owes much to Dr. Roget for having, for the first time, registered the house list, as an act of the Council.

Another circumstance of importance, I would also take the liberty of suggesting, which is, that it would be advantageous, for the Society at large, and not the Council alone, to have the power of making and repealing the bye-laws of the Society. In no other institution is this power, as far as I know, withheld from the body at large; and the inconveniences which might be feared from the proposed change, could readily be guarded against. Indeed the power of occasional appeal to the general body, is a sort of safety valve in the proceedings of a Society, which allows discussions to be held, and angry feelings to evaporate, without the necessity of giving them further publicity.

The first is, however, the most important of the changes required; and as it is of immediate practical application, I trust that it will be invariably acted upon in future. Much, no doubt, depends on the talents and public estimation of the Council, for the proper and satisfactory government of the Society's affairs; and it will be unfortunate, and not quite creditable, if such men as have it in their power to benefit the Society by their talents, should withdraw themselves from office,

fice, because their peculiar views are not immediately understood and adopted. A gentleman of conciliatory manners, with judgement, independent feelings, and energy, need not fear the accomplishment, in time, of every worthy object which he may have in view, relative to the concerns of any institution, with which he may be connected.

The paucity of high mathematical knowledge in this country is well known; and none of the few gentlemen who possess it, can well be spared from the Councils of the Royal Society. Let us hope, therefore, that they will do what they can (and they can do much), to encourage the taste for such pursuits, by their personal exertions to increase the value of the Society's Transactions. Let us flatter ourselves, that the philosopher will condescend to be the man of the world in his associations with others; and that he will aim at effecting, gradually and unobtrusively, solid and practicable improvements in the concerns of the Royal Society, to which good sense, and regard to the feelings, and even the prejudices of others, will enable him very largely to contribute. This last, it will not be beneath him to exercise; and he can well afford to employ it.

I have the honour to be, Gentlemen,
Your obedient servant,

Socius.

LV. *An Examination of those Phænomena of Geology, which seem to bear most directly on theoretical Speculations. By the Rev.* W. D. Conybeare, *M.A. F.R.S. F.G.S. &c.*

[In Continuation from p. 219.]

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

In pursuing the design, proposed in my last communication, of endeavouring to class and arrange the geological phænomena bearing more immediately on theoretical investigation, I am especially anxious that I should not be so far mistaken as to be supposed to write only, or chiefly, with controversial views. It is true, indeed, that the late work of Mr. Lyell has principally suggested to me these remarks, and that I believe the conclusions which I shall elicit, will be, in many respects, opposed to those which he has adopted: but I trust that my sole desire is impartially to follow the argument whithersoever it may lead; and that I shall be much better pleased when I shall be able to acquiesce, than when I feel constrained to differ.

In the first place, a question of method presents itself; for we may, First, commence with the effects actually resulting from the causes

causes still in operation and acting with their present power; and thus taking our departure from circumstances with which we are familiarly acquainted, we may proceed to the consideration of the geological changes produced at former periods, hoping to illustrate them by the light elicited in the course of this progress from the known to the unknown. This is the method which it is apparently the intention of Mr. Lyell to adopt; as the portion of his work as yet published is confined to the present order of things, or, as Brongniart in his late Treatise somewhat affectedly calls it, the Jovian, as distinguished from the Saturnian period. I hope I have fairly stated the advantages which this method appears to offer. The other method is, Secondly, to survey the geological phænomena, in what may be called a chronological order; beginning with those which appear to have taken place at the earliest periods, classing, as completely as we can, the effects which have been produced in the successive geological epochs in a regular order, of which the effects still in progress at the present time will of course form the last term; and finally, comparing the whole together, with the view of observing whether they all indicate a uniform and constant operation of the same causes, *acting with the same intensity*, and *under the same circumstances*; or rather evince that there has been a gradual change in these respects, and that the successive periods have often given rise to such new circumstances, as must have in a very great degree modified the original forces.

This second method, appearing to me more strictly philosophical, I shall at present adopt; but I would here for once observe, that the geologists whose cause I advocate, seem from the frequent employment of the phrases "*existing causes*," and "*the uniformity of Nature*," to lie under a misconception in Mr. L.'s mind; as though they speculated on causes of a different order from any with which we are acquainted, and almost reasoned on the supposition of different laws of nature: Whereas I conceive both parties equally ascribe geological effects to known causes, viz. to the action of water, and of volcanic power: only those with whom I class myself maintain, that much which has resulted from aqueous action, *e. g.* the excavation of many valleys, indicates rather the violent action of mighty diluvial currents, than effects which do or can result from the present drainage, by the actual rivers, of the waters descencing from the atmosphere in rain, &c. to which Mr. Lyell looks exclusively. As I shall hope to show, that on every possible geological hypothesis, which either Mr. L. or any one else can embrace, it must be allowed that such violent currents must have swept over our continents, as it is probable, at several periods;

periods; the only question, I must confess, appears to me to be, whether we prefer embracing an adequate or an inadequate cause. In like manner as to volcanic agency, we are both of us equally ignorant as to the cause of this power; but surely whatever that cause may be, there can be nothing unphilosophical in supposing that it might have been capable of acting with greater energy, when the materials constituting the crust of the earth were only beginning to be deposited, than at present, when the whole weight and resistance of the actual solid crust opposes it. Whatever were the muscular power of Atlas, he was probably capable of more violent exertion in his unfettered state, than he was after Jupiter had buried his limbs beneath the mass of Ætna, the "column of the heaven."

I shall now, then, enter on my proposed examination of those phænomena of geology which seem to bear most directly on theoretical speculations, stating each as shortly as possible, and then appending (under each article) the observations which it appears to me to suggest.

I. The phænomenon which may claim our earliest attention is the general form of the terraqueous globe as a spheroid of rotation. This undoubtedly appears to indicate, that, at the time when it assumed that figure, the mass must have been (at least in great measure) in a fluid state.

Observations.—If so, there will remain before us only the following possible alternative; at present, either the globe does, or it does not, retain this original fluidity. I will say a few words on either branch of this alternative.

If it shall be considered as most probable that the globe has now lost the fluidity which once pervaded a sufficient portion of its mass to account for this figure, the whole argument must surely at once be conceded in favour of those geologists who maintain that the causes which produced the geological convulsions acted under circumstances essentially different from the present; for nothing can well be more different than a fluid and a solid globe.

But it may be maintained, on the other hand, that the fluidity of the central mass still remains, on which the crust of the surface floats; and it may be argued that the extensive oscillations of earthquakes, often affecting such vast portions of the surface, afford countenance to this idea. On this supposition we may inquire whether this fluidity be igneous, or aqueous; but that it cannot be the latter, the ascertained specific gravity of the planet seems to demonstrate: allow it then (*argumenti causâ*) to be igneous; we shall thus have a central nucleus in a state of igneous fluidity, which has been gradually

covered during a long succession of ages by the various strata super strata forming the present crust. But if this be so, can we possibly suppose that the physical circumstances were at all similar at the earliest period, before a single deposit had incrusted this igneous mass; and at present, when it is buried beneath an accumulated crust of such vast thickness, which at once offers so great a resistance to any action of the subjacent igneous fluid; and must also in its formation have been accompanied by so great a refrigeration of the surface?

[To be continued.]

LVI. *Some Remarks on the Advantages which might be expected to be derived from the Formation of Artificial Climates in this Country; showing their Value to a numerous Class of Invalids, the Conditions which should determine their Shape, Size, Locality, Arrangement and Structure, their first Cost, annual Expense, and the probable pecuniary Benefit which would result to Persons embarking Capital in such a Speculation. By Mr.* J. S. LANGTON.*

IT is universally admitted that the English climate is too severe and too changeable for persons of delicate constitutions having a tendency to consumption or decline; hence the expediency of such invalids resorting to warmer climates, a remedy which in most cases is only to be obtained either by a total separation from all kindred and acquaintance, or by causing a whole family to leave their country, perhaps forsaking an estate, or relinquishing a business, for the exclusive benefit of one of its members. The fatigues of a long journey or voyage have to be encountered, medical advice has for a time to be suspended, and when renewed has to be procured from one who is a stranger to the constitution of his patient. If therefore a possibility exists of offering to invalids so circumstanced a climate equally mild and salubrious to that of Italy, in the midst of their relatives and friends, and within reach of their accustomed physician, it must be presumed that many would gladly avail themselves of such a blessing.

In proceeding to show how such a desideratum is to be obtained, it is obvious that to mitigate the severity of winter, or to regulate the variable temperature of other seasons, a space must be inclosed, and the separation of the air occupying that space from the external atmosphere must be so effected, that nothing injurious to health may be occasioned, either by the inclusion of noxious vapours, or by the exclusion of such portions of light and air as are conducive to health. And as the more

* Communicated by the Author.

more spacious the volume of air thus inclosed is, the more pure it will be, and the less subject to sudden variations of temperature, it would appear to be the interest of a number of families to unite in having a large inclosed space common to them all; thus not only affording themselves ample room for exercise, but rendering their habitations more cheerful, by presenting opportunities for social intercourse without a necessity for transgressing the bounds of their favoured retreat.

This leads to the consideration of what form such an establishment ought to assume. If difficulties and expenses of execution were not an object, perhaps a circle would be the best which could be devised; the difficulties, however, and waste in the construction of buildings on a circular plan are so great, and the disadvantages in furnishing them so manifest, that a square would appear on the whole to be the most convenient form that could be adopted. As to size, it should be sufficiently large to admit of a drive, that carriage exercise might be taken by those who were unequal to greater fatigue.

Three hundred feet square appears to be the least space that would afford that advantage, and also allow a small shrubbery to be appropriated to each house for seclusion and for amusement. A larger space would be desirable; but it must be remembered that not only the first outlay, but a permanent annual expense is proportioned to the size of the inclosed area, these disadvantages being increased not in proportion to the length of a side, but to the square of that length. It is obvious that such a mass of buildings could only be erected where there is an uninterrupted space of at least five hundred feet square, not subject to any thoroughfare. The situation for such an establishment should be dry and airy, and placed in that neighbourhood where it might be expected the majority of the friends and relatives of its inmates should reside, that the greater chances might be afforded them of receiving the visits and attentions of their friends; it should also be convenient for the attendance of the first medical practitioners. Fulfilling these conditions will fortunately in London produce another advantage hardly less essential, viz. a comparatively clear atmosphere, from the prevailing winds blowing from the south-west, and consequently not being contaminated with the smoke and dirt of the place until after they have passed over the dwellings of those residing westward.

In arranging a new and experimental establishment of this nature, it would be politic to have it so laid out that persons of different degrees of wealth might be able to avail themselves of its advantages, lest the risk should be incurred of portions of it being unoccupied if it included only the largest

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description of houses; for it must be borne in mind that a considerable sum must be annually expended in maintaining the required temperature: in addition therefore to the occasional suspension of rent, common to all building speculations, the proprietor would here have his quota of the general expenditure thrown upon him, aud at a time when he is least able to afford it. The society must be preserved select, not so much by heavy expenses as by well regulated by-laws, to which all the proprietors and inmates of the establishment must be subject, and without which it would be absolutely impossible for a community so circumstanced to exist. To obtain the above object without interfering with the strength or simplicity of the construction, it would be expedient that only two sizes of houses should be adopted, and that two of the smaller should, with their party wall, only equal the internal size of one of the larger; the larger being placed about the middle of each side of the square, as the most enviable site, and the smaller houses being situated towards the angles. This very great disproportion in size would fix about thirty-two feet, exclusive of strong party walls, as the most desirable length of front for each large house; a longer front would seem to occasion their being unnecessarily large, a shorter would too much contract the fronts of the smaller houses. The external angles of the square might be either wholly cut off from all communication with the interior, or they might be hotels or sets of chambers communicating with it only by passages, and subject to special restrictions. The external sides of the square would form streets, affording access for carriages to every house, that a carriage access to the interior of the square might be exclusively confined to its inhabitants; the public being either wholly shut out from the interior, or admitted on such terms, and under such regulations, as a due regard to the comforts and interests of its inhabitants should dictate.

As to construction, the houses should be firmly built, that the lateral pressure occasioned by the action of a strong wind on the vast extent of the interior roof should not disturb their stability; they should be also fire-proof, that the whole establishment may be secure from the annoyance and danger which the destruction of a single house would otherwise occasion; and as it is proposed that the interior should be filled with the choicest collection of foreign fruit-trees and other exotics, it is obvious that a violent heat might destroy plants in a single hour, which it had been the work of even centuries to bring to maturity.

The method thus proposed of heightening the interest of the

the place by blending the works of nature in their rarest and most beautiful forms with those of art when devoted to that purest and noblest of all worldly purposes,—the alleviation of human suffering, would occasion a necessity for keeping the ventilation of the houses wholly distinct from that of the area or garden, that no air which had been deprived of any portion of its salubrious properties, either from the action of vegetable life, or the decomposition of dead vegetable matter, should be inhaled, except when the enjoyment of exercise would render such a circumstance unavoidable, and when the doing so, to an extent not necessarily otherwise than infinitely trifling, could hardly even in theory be deemed disadvantageous. In roofing over this inclosed space, such materials should be used as admit of offering the least obstructions to light and ventilation; airiness of effect should be gained without sacrificing strength, and cheapness without decreasing durability: these conditions obviously suggest iron as a fit material to be largely employed, especially for all columnar supports, as being cast hollow they would also serve as spouts for conveying down the rain-water; and their slight variations of length from alternations of temperature, instead of being a disadvantage, might on the contrary be used as a self-acting means of regulating the quantity of ventilation, by having many of the glazed frames swing on their centres, and firmly connecting the short arms of levers attached to them with the bases of the columns by long rods of wood or other material not subject to practical variations of length by moderate changes of temperature: the difference of length thus obtained in the present case would be about one hundredth of an inch for every three degrees by Fahrenheit's thermometer, a maximum of height is obtained by this material with a minimum of diameter. As regards lateral connections for the purpose of giving sufficient stability to the roof to resist winds, it would be desirable to form them either of timbers fixed horizontally in right lines and crossing each other at right angles, the extreme ends abutting against the party walls of each house, or of iron ribs so curved as to admit of their acting like springs, in so adjusting themselves by extension or contraction that the distances between their ends should not vary with the temperature. Iron thus employed would unquestionably admit of more elegant forms in detail than any other material to be procured at any reasonable cost; and in such a construction what would be most beautiful in detail would be the most likely to produce the best effect as a whole. If iron was the material exclusively adopted, the roof of a glass-house in Messrs. Pellat's establishment, designed by the late Mr. Tredgold,

Tredgold, is admirably calculated for showing the principle to be borne in mind in contriving the manner of connecting it together. The vast length, however, of the sum of each series of ribs, and the considerable variations of bulk to which iron is subject from changes of temperature, would present most formidable difficulties in endeavouring to obtain stability with long connected lines of that material, particularly if those connections must be confined at each end; there would be required not only such minute accuracy as to dimensions, but also such exquisite nicety in rendering all corresponding parts exactly the same in point of flexibility and elastic power, for securing the whole from general derangement of stability by any partial excess of strength, that such a task would be hopeless in theory, and extremely doubtful as to its result in practice.

The best arrangement would appear to be to connect firmly all the tops of the columns with each other, and with the party walls of the houses, by timbers something similar in form to the yards of ships; their transverse sections should be elliptical in about the proportion of fourteen inches to thirteen, and their greatest diameter placed horizontally; stiffness might be thus most effectually and œconomically gained, as the bearings of such beams would be divided by suspension from the roof, and their own weight would prevent them from curving upwards when acted upon by a compressing strain; their suspension should be adjusted horizontal when the temperature was at a mean elevation. Upon the tops of columns thus firmly retained in their places, there would be no more difficulty in placing a number of detached roofs of iron or other material, additional labour and scaffolding excepted, than there would be in placing such roofs on the ground beneath. The glass roofs would range on a level with those of the houses, except that it might be desirable to have an additional story on the north side, to prevent the warmed strata of air being quite so rapidly carried away from the surface of the glass in the coldest weather. Each division of roof would be about thirty-three feet six inches square, and in form would be two roofs intersecting each other at right angles, thus giving to each house an avenue to the one opposite. For the skeleton framing of each division of roof, cast-iron, jointed at the ridges and points of intersection, might be appropriated in exceedingly elegant forms, with the unusually concurrent advantages of durability and cheapness. The glazed frames might be either of iron or wood; the former material would admit of their being so made as least to obstruct the light, and is the most durable; wood, however, possesses a great advantage

tage for this purpose, from being so bad a conductor of heat; the annual expenses of keeping up the required temperature would from this circumstance be reduced about eight per cent by using wood in preference to iron for this purpose.

In estimating the cost of such an establishment, it appears quite unnecessary to calculate the expense of building the houses, that expense having no reference to the peculiar expenses of the place, except their being fire-proof, which circumstance, from cast-iron being at present so remarkably cheap, and from such quantities of exactly similar castings being required, would add extremely little to the cost of constructing houses of the same class without the advantage of that protection to life and property. The first expenses peculiar to this establishment would be a glazed roof, if three hundred feet square, of rather more than two acres, its framing and supports, and a very extensive apparatus for keeping up an artificial temperature. The extent of glazed frames would not exceed one hundred thousand superficial feet; this at two shillings per foot for labour and materials would make the glazed framing amount to ten thousand pounds. The pillars, with the extending beams and skeleton frame of roof, might a little exceed five thousand pounds: and the apparatus for warming and ventilating the area would rather exceed five thousand pounds more. The total cost therefore, when painted and finished, may be expected at the outside not to exceed twenty-five thousand pounds, or about seven hundred pounds for each double house; and this sum would doubtless allow a sufficient surplus with which to stock the ground, at least with vines, which intwining themselves up the columns and crossing the spaces above, would afford shade by their foliage as well as refreshment by their fruit. As for plants of rarity and value, it may be found that individual liberality may render the application of capital for their purchase wholly uncalled for, nor should the means be wanting of permanently recording acts of that nature.

The annual expense of keeping up the required temperature will usually be in London about fourteen hundred pounds; each foot of glazed surface requiring about one-fourth of a bushel of coals per annum. This calculation assumes the temperature to be kept up to about fifty-two degrees Fahrenheit during the winter months; it also supposes that no more ventilation is allowed in very cold weather than what escapes between the laps of the glass when well fitted, more ventilation being only allowed for in mild weather; while in hot weather most of the roof might be opened. The total additional expense, including interest for money sunk, may therefore

therefore be estimated at about seventy-five pounds per annum on each double house, a sum but trifling when compared with the money at present sacrificed in obtaining that degree of warmth and regularity of temperature which the establishment above described may be anticipated to supply. It may also be fairly expected that such an inclosed space would be capable, under good management, of producing fruit to no inconsiderable amount; and the privacy of the houses would be so secured by the appropriation of a small shrubbery to each, that, without materially detracting from the comfort of the place, a considerable revenue might be derived both from persons in the vicinity taking annual tickets for the privilege of promenading there in severe weather, and from the admission of strangers for the gratification of their curiosity.

Persons embarking capital in such a speculation, would, if successful, possess a description of property endowed with such peculiar advantages as it would be impossible to compete with by their extension to any house property already erected; while even assuming that from prejudice or some cause not capable of being anticipated it should be deemed expedient to uncover the interior, they would still possess a delightful square, surrounded by excellent houses, having its grounds perfectly retired, and secure from those violent currents of wind to which squares laid out in the usual manner are so peculiarly liable. Nor is it to be supposed that Government would look with apathy upon a speculation, which would not only be a convenience to the public, but which, as far as it was carried, would have a direct tendency to decrease the number of absentees. There is probably no speculation to which the Government would be more disposed to grant assistance in the way of a charter, particularly as it would be in perfect accordance with the prescribed rule which restricts charters to those undertakings which cannot be conducted with advantage by individual enterprise.

Langton, near Spilsby, Lincolnshire. JOHN STEPHEN LANGTON.

LVII. *On the Arabic Names of the Stars. By* A CORRESPONDENT.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

THE names of the stars inserted on celestial globes and maps are generally such violent corruptions of the Arabic names for which they are intended, that perhaps the following catalogue (with Bayer's Greek Letters annexed) of the names given

given by Ulugh Beigh, with Hyde's Latin Interpretation, may be interesting to some of your readers.

Ad Ursam Minorem.

α *Gjedi*, Hædus: vel *Caucab Shemâli*, Stella Borealis.
η *Anwer Al Pherkadein**.
ζ *Anpha Al Pherkadein.*

Ad Ursam Majorem.

ι *Al Phik'ra Al Thâlitha*, Vertebra tertia.
α *Duhr Al Dub Al Acber*, Dorsum Ursi Majoris.
β *Merâk Al Dub Al Acber*, Epigastrium Ursi Majoris.
δ *Meg'rez Al Dub Al Acber*, Uropygium (Insitio Caudæ) Ursi Majoris.
γ *Phacd Al Dub Al Acber*, Femur Ursi Majoris.
Hæ quatuor α, β, δ, γ constituunt *Al Na'sh Al Cub'ra*, Feretrum Majus.
λ *Al Phik'ra Al Thânija*, Vertebra secunda.
ν *Al Phik'ra Al Ula*, Vertebra prima.
ε *Al Gjaun* .... vel *Aly'a*, Cauda.
ζ *Al Inâk* } *Al Benât*, Filiæ, sc. Feretri.
η *Al Kâïd*
Cabd Al Asad, Jecur Leonis.

Ad Draconem.

μ *Al Râkis*, Saltator.
ν *Al Awâïd*, Pulsatores Testudinis.
γ *Râs Al Tinnîn*, Caput Draconis.
σ *Al Athâphi*, Chytropodes.
Adphâr Al Dîb, Ungulæ Lupi.
ζ *Al Dîb*, Lupus.
ο *Al Dîch*, Hyæna.

Ad Cepheum.

γ *Al Râï*, Pastor.
β *Cawâkib Al Phirk*, Stellæ Gregis.

Ad Boötam.

η *Muphrid Al Râmih*, Efferens Inermem.
α *Simâk Al Râmih*, Efferens Hastiferum; Arcturus; in Ptolemy Ἀρκτοῦρος.

Ad Coronam.

α *Naïr Al Phecca*, Lucida Pheccæ; vulgò *Alpheta.*

Ad Herculem.

α *Râs Al Gjâthi*, Caput Ingeniculi.

* *Al Pherkadein*, Duo Vituli. Hyde does not give the interpretation of the words *Anwer* and *Anpha.*

Ad Lyram.

α *Al Nesr Al Wâki,* Vultur cadens; vulgò *Vega.*

Ad Cycnum.

β *Minkâr Al Degjâgje,* Rostrum Gallinæ; vulgò *Albireo.*
γ *Sadr Al Degjâgie,* Pectus Gallinæ.
α *Al Ridph,* Quæ ponè est: vel
Danab Al Degjâgie, Cauda Gallinæ.
ω *Rucba Al Degjâgje,* Genu Gallinæ.

Ad Cassiopeiam, vel Inthronatam.

α *Dât Al Cúrsa,* Inthronata (called so κατ' ἐξοχήν): vel
Sadr, Pectus, vulgò *Schedir.*
β *Caph Al Chadîb,* Manus tincta.

Ad Perseum.

Mi'sam Al Thuraiyâ, Corpus Pleiadum.
α { *Marphak Al Thurayâ,* Cubitus Pleiadum: vel
Gjemb Bershâush, Latus Persei. }
β *Râs Al Ghûl,* Caput Larvæ; vulgò *Algol.*
ξ *Menkib Al Thuraiyâ,* Humerus Pleiadum.
ο *Atik Al Thuraiyâ,* Interscapilium Pleiadum.

Ad Aurigam.

α *Al Aijûk,* Capella: ὁ 'Αἴξ in Ptolemy.
β *Ménkib dil Inân,* Humerus Aurigæ.
γ *Ca'b dil Inân,* Talus Aurigæ.

Ad Ophiuchum.

α *Al Râï,* Pastor: vel *Râs Al Hauwâ,* Caput Ophiuchi, vulgò *Ras Al Hangue.*
β *Kelb Al Râï,* Canis Pastoris.

Ad Serpentem.

α *Unuk Al Háiya,* Collum Serpentis.

Ad Aquilam.

α *Al Nesr Al Taîr,* Vultur volans; vulgò *Altair.*
ε *Danab Al Okâb,* Cauda Aquilæ.

Ad Delphinum.

ε *Danab Al Dulphîn,* Cauda Delphini.

Ad Pegasum.

δ Peg. vel α And. { *Sírra Al Phâras,* Umbilicus Equi: vel
Râs Al Mar'a Al Mosalsala, Caput Mulieris catenatæ; vulgò *Alpherat.* }
γ *Gjenâb Al Phâras,* Ala Equi; vulgò *Algenib.*
β *Menkib Al Phâras,* Humerus Equi; vulgò *Scheat,* which is a corruption of *Sa'd.*
α *Matn Al Phâras,* Lumbus vel Dorsum Equi.

τ *Al Kerb,*

τ *Al Kerb*, vel *Al Kereb*, Funis qui ad mediam Urnæ alligatur.
η *Sa'd Mátar*, Fortuna Pluviæ.
μ *Sa'd Bâri*, Fortuna præcellentis.
ζ *Sa'd Al Homâm*, Fortuna Herois.
θ *Sa'd Al Bahâïm*, Fortuna Bestiarum.
ε { *Phám Al Pháras*, Os Equi: vel *Gjáhphela Al Pháras*, Labium Equi. }

Ad Andromedam.

β { *Gjemb Al Mosálsala*, Latus catenatæ: vel *Betn Al Hút*, Venter Piscis: vel etiam *Mizar*, Subligaculum, vulgò *Mirach*. }
γ *Rigjl Al Mosálsala*, Pes catenatæ: vel *Anâk Al Ard*, Melis, taxo, vulgò *Alamach*.

Ad Triangulum.

β *Râs Al Mothállath*, Caput Trianguli.

Ad Arietem.

γ & β *Al Sheratein*, the dual from *Shérat*, Signum; vulgò *Mesartim*.
ε & ϱ *Min Botein*, De Botein. *Botein* is a diminutive from *Betn*, venter.
α *Al Nâtih*, Cornupeta.

Ad Taurum.

Al Debarân, The Hyades; Quicquid ponè, posteriùs, et à tergo est.
α *Ain Al Thaur*, Oculus Tauri; vulgò *Aldebaran*. In Ptolemy, ὁ λαμπρὸς τῶν Ὑάδων.
d *Wásat Al Thuraiyâ*, Medium Pleiadum. *Thuraiya* is a diminutive form from *Thérwa*, multus, copiosus.
Al Thuraiya, The Pleiades. In Ptolemy, ἡ Πλειάς. In Homer we find,

Πληϊάδας τ' ἐσορῶντι καὶ ὀψὲ δυόντα Βοώτην.

And again, in Hesiod,

Πληϊάδας θ' Ὑάδας τε, τότε σθένος Ὠρίωνος.

Ad Geminos.

α *Râs Al Tawum Al Mukáddem*, Caput prioris Gemellorum; Castor.
β *Râs Al Tawum Al Muácchcr*, Caput posterioris Gemellorum; Pollux.
γ *Al Hen'a*. *Hena* est quævis res quæ aliam sequitur vel alteri superstruitur.

Ad Cancrum.

Ma'laph Al Néthra, Præsepe; ἡ Φάτνη in Ptolemy.
α & δ *Al Hamarein*, Duo Asini.

Ad Leonem.

Minchir Al Asad, Nares Leonis.
ε *Râs Al Asad Al Shemâli*, Caput Leonis Boreale.
δ *Râs Al Asad Al Gjenûbi*, Caput Leonis Australe.
Al Gjeb'ha, Frons.
α *Kalb Al Asad*, Cor Leonis; ὁ Βασιλίσκος in Ptolemy; vulgò Regulus.
α *Duhr Al Asad*, Dorsum Leonis.
θ & κ *Min Al Zub'ra*, De Crine Dorsi.
β *Serpha*, Mutatrix.
Daphîra Al Asad, Cirrus Leonis.
h *Al Daphîra*, Cirrus plexus et in se contortus.

Ad Virginem.

β & δ *Min Al Auwâ*, De Latratore.
η *Zâwiya Al Auwâ*, Angulus Latratoris.
ε *Mukdim Al Ketûph*, Antevindemiator, vulgò Vindemiatrix; in Ptolemy Προτρυγητής.
α *Simâk Al A'zal*, Efferens Inermem, vulgò Spica; in Ptolemy Στάχυς.
ν *Min Al Gaphr*. *Ex Al Gaphr*, quod significat ventrem, velamen et tecturam.

Ad Libram.

α *Al Kiffa Al Gjenubiya*, Lanx Australis.
β *Al Kiffa Al Shemâliya*, Lanx Borealis.

Ad Scorpionem.

β *Iclîl Al Gjeb'ha*, Corona Frontis.
ω *Gjeb'ha Al Akrab*, Frons Scorpionis.
α *Kalb Al Akrab*, Cor Scorpionis; Antares; in Ptolemy Ἀντάρης.
λ *Shaula*, Cauda Scorpionis.
Tâli Al Shaula, Sequens Caudæ Scorpionis.

Ad Sagittarium.

γ *Min Al Naâïm Al Wârida*, E Pecoribus adeuntibus.
σ *Min Al Naâïm Al Sâdira*, E Pecoribus redeuntibus.
υ *Ain Al Râmi*, Oculus Sagittarii.
ψ *Min Al Naâïm Al Sâdira*, E Pecoribus redeuntibus.
β *Urkûb Al Râmi*, Suffrago seu Magnus Tendo Sagittarii.
α *Rucba Al Râmi*, Genu Sagittarii.

Ad Capricornum.

β *Min Sa'd Al Dâbih*, Ex Fortunâ mactantis.

γ *Sa'd*

γ *Sa'd Nâshira*, Fortuna averruncantis nuncium.
δ *Danab Al Gjedi*, Cauda Capricorni.

Ad Aquarium.

α *Sa'd Al Melik*, Fortuna Regis.
β *Sa'd Al Süûd*, Fortuna Fortunarum.
ν *Sa'd Búla'*, Fortuna deglutientis.
ε *Sa'd Al Achbiya*, Fortuna tentoriorum.
α *Diphda Auwal*, Rana prima: vel
Pham Al Hût AlGjenûbi, Os Piscis Australis; vulgò *Fomalhaut*.

Ad Cetum.

γ *Caph Al Gjedmâ*, Manus truncata.
π *Danab Ketus Shemâli*, Ceti Cauda Borealis.
η *Danab Al Gjenûbi*, Cauda Australis: vel
Al Diphda Al Thâni, Rana secunda.

Ad Orionem.

λ *Hek'a*, Circulus vel album quiddam emicans in superiori parte pectoris equi.
α *Menkib Al Gjauzâ*, Humerus Orionis: vel
Jed Al Gjauzâ Al Júmna, Manus dextra Orionis; vulgò *Betelgeux*.
γ *Al Mîrzam Al Nâgjid*, Leo strenuus.
δ & ε *Al Tâgi* et *Al Dawâib*, Tiara et Antiæ.
δ *Mintaka Al Gjauzâ*, Cingulum seu Baltheus Orionis.
η *Saiph Al Gjebbâr*, Ensis Gigantis.
β *Rigjl Al Gjauzâ Al Jusra*, Pes Orionis sinister; vulgò *Rigel*.
κ *Rigjl Al Júmna*, Pes dexter.

Ad Eridanum.

α *Al Dalîm*, Aggesta Terra tanquam Agger: vel
Acher Nahr, Ultima Fluminis, vulgò *Acarnar*.

Ad Leporem.

α *Arsh Al Gjauzâ*, Solium Orionis.

Ad Canem Majorem.

α *Al Shira AlJemâniya*, Sirius Jemanensis; Sirius; in Ptolemy ὁ *Κύων*; in Hesiod *Σείριος*,

Εὖτ' ἂν δ' Ὠρίων καὶ Σείριος ἐς μέσον ἔλθῃ
Οὐρανὸν, Ἀρκτοῦρον δ' ἐσίδῃ ῥοδοδάκτυλος Ἠώς.

β *Al Mirzam?*

Ad Canem Minorem.

β *Al Mirzam?*
α *Al Shirâ Al Shâmiya*, Sirius Shamensis; Procyon; in Ptolemy ὁ *Προκύων*.

Ad

Ad Navem.

α *Soheïl*, a diminutive from *Sahala*, tenuis, facilis, ac planus fuit; Canopus; in Ptolemy Κάνωβος.

Ad Hydrum.

σ *Minchir Al Shugjâ*, Nares Hydri.
α *Unuk Al Shugjâ*, Collum Hydræ.

Ad Corvum.

α *Minkâr Al Gorâb*, Rostrum Corvi.
γ *Gjenâh Al Gorâb Al Aiman*, Ala dextra Corvi.

Ad Centaurum.

Rigjl Kentaurus, Pes Centauri.

In some cases the name given on our globes to a single star is the Arabic name of the whole Constellation, as α Andromedæ, *Alpherat*, a corruption of *Al Pharas*, the Horse, the name for the constellation Pegasus; α Ursæ Majoris, *Dubbe*, from *Al Dub*, the Bear. In other instances the names are so much corrupted that it is difficult to trace their origin: as α Orionis, of which the Arabic name is *Jed Al Gjauzâ Al-Júmna*, the Right Hand of Orion, and which is called on our globes *Betelgeux*, in the Alphonsine Tables *Beldelgenze*.

Of some of the names on the globes we are unable to trace the origin, as *Mesartim*, *Albireo*; and the meaning of some of the interpretations given by Hyde, is not very clear.

"The Arabic names of the stars are to be differently considered according as they are more or less ancient. The less ancient are the names of signs, and parts of the same, such as the limbs of the animals and the like, which have been transplanted into the Arabic from the Greek. But the more ancient are the names of some conspicuous stars occurring here and there in many of the constellations, which have not been received from the Greeks, but were devised and given originally and in remote antiquity by the Arabs themselves. Of this kind also are the names of the lesser constellations which are found in the greater ones, as *Auwâ* in the Virgin, *Benat Al Na'sh* in the Great Bear, *Al Himarein* in Cancer, and many others, which were altogether unknown to the Greeks." *Hyde, Comment. p.* 5. Some of the names appear to have reference to Astrology, as *Sa'd Al Homâm*, Fortuna Herois, &c.

The Arabic names of the *constellations* in Ulugh Beigh's Catalogue are translations of the Greek names in Ptolemy: in one instance, however, they appear to have fallen into error by supposing Boötes to be ἀπὸ τοῦ βοᾷν, *à clamando*, wherein they call this constellation *Auwâ*, Vociferator.

LVI. *No-*

LVIII. *Notices respecting New Books.*

Flora Devoniensis: or A descriptive Catalogue of Plants, growing wild in the County of Devon, arranged both according to the Linnæan and Natural Systems, with an account of their geographical distribution, &c. By the Rev. J. P. Jones *and* J. F. Kingston.

UNDER this title we have to introduce to the notice of our readers a Flora of one of the most interesting of our English counties, distinguished, as it is justly observed, "by the extent of its coasts, the variety of its soils, and the diversity of its surface." We extract from the Preface a passage which will best show the nature of the undertaking:

"The work is divided into two parts; the first exhibiting the Linnæan, the second the Natural method of arrangement: under the latter, the different orders of the cryptogamous plants will be found carefully distributed according to the latest investigations and discoveries.....A minute attention has been paid to the essential characters of the genera and species, and any thing like repetition or superfluous observations studiously avoided. The references have also been limited as much as possible to a few of the best and most accessible works on the subject, and have only occasionally been increased where any confusion or discrepancy in the names or descriptions seems to have obviously required it; so that we trust the arrangement of the whole has been rendered as simple and concise as it could well be, without incurring the charge of deficiency or obscurity.

"To complete the subject, the relative proportions of the different natural families and their geographical distribution in the county have been added at the end of the second part."

In addition to this we may add, that the localities of the different plants appear to be very full and precise, and to have received all the attention that so important a feature in works of this sort requires.

A good clear outline of the extent and geological character of the county, and summary views of vegetable geography generally, and that of Great Britain in particular, are introduced, as well as a comprehensive statement of the number of species in each natural family included in the County Flora, and the relative proportions they bear to each other; thus clearly illustrating the vegetable character of the county, and showing its riches and deficiencies; whilst at the same time useful data are given for instituting more general comparisons and drawing more comprehensive deductions.

We could have wished that the authors of this work, as they follow Sir James Smith in their general arrangement, had consulted his last work, "The English Flora:" but we do not perceive a single reference to this performance, nor to his new genera or species. We would not have quarrelled with them if they had rejected his alterations after due examination; but we really fear they have not looked into his more recent labours, and that they have contented themselves with "English Botany," and some early edition of his "Compendium."

We think too they have not acted discreetly in printing twice over the specific characters and *habitats* of the phænogamous plants in order

order to present the reader with the natural arrangement. The whole book indeed is printed far too expensively for a local Flora; and the authors probably would have done themselves, as well as the public, more justice, if they had condensed their matter within a smaller compass. It is the extensive diffusion of science that will do good to the present generation; and to effect this, the spirit of a book should not be diluted too copiously.

But after all, we may venture to recommend this work to those lovers of Nature who are resident in Devon, and to the numerous visitors that its romantic and picturesque scenery and genial climate are constantly attracting to its shores.

Observations on the Genus Unio, together with Descriptions of Eighteen New Species; and of the Genus Symphynota, now separated from the Family of Naïades, containing Nine Species. Read before the Am. Phil. Soc. November 1827 *and March* 1829, *and published in their Transactions, vol.* iii. *N. S.* By Isaac Lea, M.A.P.S. M.A.N.S. &c. *Philadelphia:* 1829. *4to, pp.* 71. *Eight coloured engravings.*

[In the Phil. Mag. and Annals, N.S. vol. iv. p. 372, we gave the characters of six new species of Unio, as described by Mr. Lea in a former paper: we shall now present to our readers the characters of the shells described by this zealous conchologist in the memoir before us, prefacing them with an extract from his introductory remarks relating to the arrangement and groups of the *Naïades*.]

It is the opinion of some eminent conchologists that the family of the *Naïades* possesses but one genus, and that the genera into which it is at present divided are only species, and the species varieties. Were we to adopt this division, we should be in a worse dilemma than before; for we can scarcely imagine bivalves more different from each other in form than are some of our trans-Alleghany species of Unio.

How totally different is the *rectus* of Lamarck from the *irroratus?* (nobis). The first is four times the width of its length, whilst the latter is longer than broad. The one is broad rayed, in fine specimens; the other possesses dotted lines universally. The *triangularis* of Barnes is entirely dissimilar to the *nasutus* of Say, as is also the *circulus*, herein described, from the *lanceolatus* (nobis); and the same may be said of *plicatus* and *pictorum*. Two species could be scarcely more unlike than the smooth and radiated *siliquoideus* of Barnes and the beautiful tuberculated *lacrymosus* (nobis); and the same remark may be applied to the *cylindricus* and *alatus* of that excellent conchologist Mr. Say. Many other species could be thus contrasted, but I deem the above sufficient, upon examination, to prove the justness of my remarks, and the necessity, in the present state of our knowledge, to retain the species, whatever may be the changes in the genera *.

7. Unio

* In a letter addressed to me by William Cooper, Esq., an intelligent naturalist of New York, he says, "There are now, I think, not less than thirty North American species of Unio well established, and perhaps seven or eight more. That they are species, each perpetuating its peculiar form, subject

7. Unio ater.—*Testâ ovatâ, inæquilaterali, transversâ, ventricosissimâ; umbonibus elevatis; natibus prominulis; epidermide rugosâ nigrâque; umbonibus elevatis; dentibus cardinalibus erectis, cristatis, lateralibus granulatis, rectisque; margaritâ roseâ.*

Hab. Mississippi below Natchez. T. W. Robeson. My cabinet. Cabinet of Prof. Vanuxem. Diam. 2·6, length 3, breadth 4·5 inches.

8. Unio rubiginosus.—*Testâ inæquilaterali, transversâ, postice sub-biangulari, antice rotundatâ; valvulis sub-crassis; natibus prominentibus, recurvis, postice sub-angulatis; dente cardinali magno, laterali crasso; margaritâ salmonis colore.*

Hab. Ohio. My cabinet. Cabinet of T. G. Lea. Cabinet of Prof. Vanuxem. Cabinet of the Academy of Natural Sciences. Diam. 1·2, length 2·1, breadth 2·6 inches.

9. Unio Heterodon.—*Testâ rhomboido-ovatâ, inæquilaterali, ventricosâ; valvulis tenuibus; dentibus cardinalibus compressis, latis; dentibus lateralibus sub-curvatis, dente laterali valvulæ dextræ, duplici; natibus prominentibus; ligamento sub-brevi; margaritâ albâ.*

Hab. Schuylkill and Derby Creek, Pa. My cabinet. Cabinet of Mr. Mason. Cabinet of Prof. Vanuxem. Cabinet of Dr. Griffith. Cabinet of the Academy of Natural Sciences. Cabinet of Mr. Hyde. Cabinet of Mr. Phillips. Cabinet of Mr. Conrad. Diam. ·5, length ·9, breadth 1·5 inch.

10. Unio sulcatus.—*Testâ sub-ellipticâ, inæquilaterali, ventricosâ, sub-emarginatâ; valvulis crassis; natibus fere terminalibus; dentibus cardinalibus lateralibusque magnis, et duplicibus in valvulis ambabus; margaritâ purpureâ.*

Hab. Ohio. T. G. Lea. My cabinet. Cabinet of T. G. Lea. Cabinet of Prof. Vanuxem. Cabinet of P. H. Nicklin. Cabinet of the Academy of Natural Sciences. Diam. 1·3, length 1·7, breadth 2·3 inches.

11. Unio planulatus.—*Testâ inæquilaterali, ovato-ellipticâ, transversâ; complanatâ per umbones à natibus usque ad marginem inferiorem, maculis quadratis radiatim pictâ; natibus prominulis; dente cardinali parvo, laterali magno, crasso, curvato; margaritâ sub-cæruleo-albâ.*

Hab. Ohio. T. G. Lea. My cabinet. Cabinet of T. G. Lea. Ca-

subject to certain variations, but permanent within fixed limits, seems to me the most rational opinion, although some of our most judicious naturalists think otherwise. Your account of the animal of the *U. irroratus* affords a strong argument in favour of this belief; for it proves that to be, beyond doubt, as distinct a species as any in any class of animals. Yet this may always be known with certainty by the shell, which, though so well characterized, is not, however, more different from the rest of the genus, than they are from each other, and frequently still less so. If, therefore, this difference is found to be constantly indicative of a species in one instance, it must also be in others. I believe that our lakes and rivers contained the same form of shells at the creation and ever since that they do at this day. If they are hermaphrodite per se, as is said of them, it could not be otherwise; and if the contrary were admitted, natural history would not deserve the name of a science."

binet of Prof. Vanuxem. Cabinet of P. H. Nicklin. Cabinet of the Academy of Natural Sciences. Diam. ·8, length 1·3, breadth 2·2 inches.

12. Unio Circulus.—*Testâ circulari, ventricosâ, sub-æquilaterali; valvulis crassis; natibus prominulis; dentibus cardinalibus lateralibusque magnis; ligamento brevi crassoque; margaritâ albâ et iridescente.*

Hab. Ohio at Cincinnati. T. G. Lea. Monongahela at Pittsburg. T. Bakewell. Tennessee at Nashville. Prof. Vanuxem. My cabinet. Cabinet of T. G. Lea. Cabinet of Prof. Vanuxem. Cabinet of P. H. Nicklin. Cabinet of Dr. Griffith. Cabinet of W. Hyde. Cabinet of W. Mason. Cabinet of J. Phillips. Cabinet of the Academy of Natural Sciences. Cabinet of Peale's Museum, &c. *Unio rotundata?* Lamarck. Diam. 1, length 1·5, breadth 1·5 inch.

13. Unio multi-radiatus.—*Testâ ellipticâ, inæquilaterali, ventricosâ, multi-radiatâ; valvulis tenuibus; natibus prominulis; dentibus cardinalibus erectis, et in valvulis ambabus duplicibus; lateralibus lamelliformibus et abruptis; margaritâ cæruleo-albâ.*

Hab. Ohio. T. G. Lea. My cabinet. Cabinet of T. G. Lea. Cabinet of Prof. Vanuxem. Diam. ·8, length 1·3, breadth 2 inches.

14. Unio Occidens.—*Testâ sub-ellipticâ, inæquilaterali, transversâ, ventricosâ; valvulis crassis; natibus sub-undulatis, raro decorticatis; ligamento sub-brevi crassoque; dentibus elevatis; margaritâ albâ.*

Hab. Ohio. T. G. Lea. My cabinet. Cabinet of T. G. Lea. Cabinet of Prof. Vanuxem. Cabinet of P. H. Nicklin. Cabinet of the Academy of Natural Sciences. Cabinet of Peale's Museum. Diam. 1·6, length 2·3, breadth 3·4 inches.

15. Unio Securis.—*Testâ sub-triangulari, inæquilaterali, per umbones valde complanatâ; valvulis crassis; natibus elevatis, recurvatis, compressissimisque; dente cardinali magno, laterali crasso; ligamento breviusculo, crassoque; margaritâ albâ et iridescente.*

Hab. Ohio. T. G. Lea. My cabinet. Cabinet of T. G. Lea. Cabinet of Prof. Vanuxem. Cabinet of the Academy of Natural Sciences. *Unio depressa* of Rafinesque. Diam. ·9, length 1·5, breadth 1·9 inch.

16. Unio Iris.—*Testâ angusto-ellipticâ, inæquilaterali, sub-ventricosâ; valvulis tenuibus; natibus prominulis; dente cardinali in valvulâ sinistrâ, duplici, in dextrâ sub-bifido, parvo, erecto; dentibus lateralibus longis tenuibusque; margaritâ sub-cæruleo-albâ.*

Hab. Ohio. T. G. Lea. My cabinet. Cabinet of T. G. Lea. Cabinet of Prof. Vanuxem. Diam. ·5, length ·8, breadth 1·6 inch.

17. Unio Zig-zag.—*Testâ ovatâ, inæquilaterali, ventricosâ; valvulis sub-crassis; dentibus cardinalibus magnis, erectis; lateralibus curvatis; natibus prominulis; radiis ex lineis angulatis compositis; ligamento brevi crassoque; margaritâ albâ.*

Hab. Ohio. T. G. Lea. My cabinet. Cabinet of T. G. Lea. Cabinet of Prof. Vanuxem. Cabinet of P. H. Nicklin. Cabinet of the Academy of Natural Sciences. Cabinet of Peale's Museum. Diam. ·6, length ·9, breadth 1·5 inch.

18. Unio

18. Unio patulus.—*Testâ ovatâ, compressâ, cuneiformi, inæquilaterali, obliquâ, transversâ; umbonibus compressis; valvulis sub-crassis; natibus sub-terminalibus; dente cardinali parvo; laterali longo et subcurvato; margaritâ albâ.*

Hab. Ohio. T. G. Lea. My cabinet. Cabinet of T. G. Lea. Cabinet of Prof. Vanuxem. Diam. ·8, length 1·4, breadth 2·3 inches.

Genus SYMPHYNOTA.

Testâ fluviatili, bivalvi; valvulis supernè connatis.

Shell fluviatile, bivalve; valves connate at the dorsal margin

Animal same as that of Unio. [?]

Remarks.—Objections will most likely be made to the introduction of a new genus into a family acknowledged already to be in great confusion, and presenting many and various difficulties. The formation of the genus *Symphynota*, it is hoped, will rather be conducive to a diminution of that difficulty, by a division which all must acknowledge to be as natural as any of those of the family. The distinctive characteristic of this genus is the testaceous connection of the two valves of the shell above the hinge. I therefore remove from the existing genera all the connate shells without regard to the forms of their teeth, believing, that should this family be hereafter remodelled, it will present only two natural genera; one having a testaceous connection of the valves, the other dispossessed of it. The difficulties attending the adopted genera of the *Naïades*, viz. *Unio* of Bruguière, *Hyria, Anodonta, Iridina, Castalia** of Lamarck, *Dipsas* of Leach, and *Alasmodonta* of Say, have been mentioned by two eminent English conchologists, W. Swainson and G. B. Sowerby, as well as in America by P. H. Nicklin. Mr. Sowerby (Zool. Journ. vol. i. p. 55.) has reunited them under the name of *Unio*, of which he makes two great divisions. 1. Without teeth. 2. With teeth; and these are each subdivided into "winged" and "not winged;" which are again divided into the various forms of teeth, or the "hinge line." The evident objection to this arrangement is the difficulty of deciding upon the passage from the "not winged" to the "winged." Thus we do not find the *Anodonta trapezialis* and *Anodonta glauca*, which Lamarck describes as "*compresso-alatâ*," mentioned among the "winged," while we have "*Anodon alatus* of Swainson and Lamarck," which is not described in the "Hist. Nat. des Animaux sans Vertèbres †."

It is evident that the apparatus for depositing the calcareous and epidermidal matter on the elevated and connected wing must be differ-

* This genus was placed by Lamarck in the family *Trigonæa*, certainly with no propriety. It has been placed by Sowerby and Latreille among the *Naïades*, and here must be considered as a species of Unio, and not a genus. The observant M. de Blainville has placed *Castalia* and *Hyria* among the *Uniones*, and *Iridina* and *Dipsas* among the *Anodontæ*. *Castalia ambigua* is undoubtedly a fluviatile shell, and approaches most closely to the *U. triangularis*. The teeth are those of the Unio, and it differs only in its longitudinal furrows from the general characters of the Unio.

† Say describes his *An. gibbosa* as being alated.

ent from that of the inhabitant of free valves, to which it has been denied by Nature.

Lamarck and Barnes both mention in their description of the *U. alatus* of Say, that M. Le Sueur thought this shell should constitute a new genus. Since that time so many connate shells have come to my notice, that I feel satisfied the science of conchology will be subserved by the institution of this natural genus, which will embrace, in all probability, several others, viz. *Hyria* of Lamarck, *Dipsas* of Leach, and *Cristaria*, *Prisodon*, and *Paxyodon* of Schumacher, all of which, when they shall be found perfect, will most probably turn out to be connate shells. Lamarck suspected his *Hyria* to be connate, like the *U. alatus*; for when describing that species, he says, "Nos *Hyries* auraient-elles une pareille réunion à la carène de leur corselet?" Indeed the fact can scarcely be doubted.

1. Symphynota lævissima.—*Testâ ovato-triangulari, inæquilaterali, transversim rugosâ, sub-ventricosâ; valvulis tenuissimis, superne bi-alatis, ante et post nates connatisque; dentibus cardinalibus et lateralibus lineam curvatam facientibus; natibus prominulis; ligamento celato; margaritâ purpureâ et iridescente.*

Hab. Ohio. T. G. Lea. My cabinet. Cabinet of T. G. Lea. Cabinet of Prof. Vanuxem. Cabinet of P. H. Nicklin. Diam. 1·4 inch., length from beaks to base 2·4 inches, breadth 4·5 inches, length from the top of the wing to base 3·1 inches.

2. Symphynota bi-alata.—*Testâ ovato-triangulari, inæquilaterali, transversim rugosâ, sub-ventricosâ; margine dorsali bi-alatâ; valvulis tenuibus, ante et post nates connatis; natibus et alæ posterioris basi apiceque undulatis; natibus haud prominentibus; dente lamelliformi unico in valvulâ utrâque; ligamento celato; margaritâ tenui et iridescente.*

Hab..... fresh waters of the south of Asia? Brought from Canton by Captain Barr. My cabinet. Cabinet of Mr. Pierpoint. Cabinet of Mr. Hyde. Cabinet of Mr. Phillips. Diam. 1 inch, length from the beaks to the base 2 inches, breadth 3·6 inches, length from the top of wing to base 3·4 inches.

3. Symphynota alata.—*Testâ ovato-triangulari, transversim rugosâ, sub-compressâ; valvulis crassiusculis, earum marginibus dorsalibus alatis, et super ligamento connatis; dente cardinali in valvulis ambabus duplici, laterali in sinistrâ tantum duplici, sub-curvato; ligamento sub alâ celato; natibus prominulis; margaritâ purpureâ.*

Hab. our western waters. *Unio alatus*. Say. Nicholson's Encyclopædia (Am. Ed.) Art. Am. Conch. pl. 4. fig. 2. *Unio alata*. Lamarck. *Unio alatus*. Barnes. Silliman's Am. Journ. vol. vi. *Unio alata*. Swainson. Diam. 2, length 4·7, breadth 6·9 inches.

4. Symphynota complanata.—*Testâ ovato-triangulari inæquilaterali, transversim rugosâ, compressâ; valvulis crassis; margine posteriori dorsali alatâ connatâque; dente unico cardinali in valvulâ utrâque; plano irregulari calloso sub ligamento; natibus compressis, sub-prominulis; ligamento celato; margaritâ albâ, iridescenti.*

Hab. Fox River. Mr. Schoolcraft. Wisconsan. Captain Douglass. Ohio. W. Cooper. My cabinet. Cabinet of Mr. Barnes. Cabinet of Prof.

Prof. Vanuxem. Cabinet of the New York Lyceum. Cabinet of Dr. Mitchill. Cabinet of the Academy of Natural Sciences. *Alasmodonta complanata.* Barnes. Diam. ·9—1·4 inch, length from beaks to base 3 inches, breadth 5 inches, length from the top of the wing 4·3—4·5 inches.

5. Symphynota compressa.—*Testâ transversim elongatâ, inæquilaterali, valde compressâ, ellipticâ; valvulis tenuibus; natibus subprominulis, undulatis; dente cardinali prominente; laterali parvo.*

Hab. Ohio. T. G. Lea. Norman's Kill, near Albany. Dr. Eights. My cabinet. Cabinet of Prof. Vanuxem. Cabinet of Dr. Eights. Cabinet of P. H. Nicklin. Cabinet of the Academy of Natural Sciences. Cabinet of the New York Lyceum. Diam. ·8, length 1·7, breadth 2·8 inches.

6. Symphynota gracilis.—*Testâ sub-triangulari-ovatâ, inæquilaterali, transversim rugosâ, sub-compressâ; valvulis tenuibus fragilibusque; margine posteriori dorsali sub-alatâ, connatâque; dente cardinali in valvulâ dextrâ elevato, recurvo; natibus sub-prominulis; ligamento celato; margaritâ violaceo-purpureâ et iridescente.*

Hab. Ohio. T. G. Lea. Wisconsan. Mr. Schoolcraft. My cabinet. Cabinet of Mr. Barnes. Cabinet of Prof. Vanuxem. Cabinet of P. H. Nicklin. Cabinet of Mr. Swainson. Cabinet of the New York Lyceum. Cabinet of the Academy of Natural Sciences. *Unio gracilis.* Barnes. Silliman's Amer. Journ. vol. vi. p. 174. *Unio fragilis.* Swainson. *Unio planus.* Barnes. Diam. 1—1·2, length 2·2—2·5 breadth 3·1—4·1 inches.

7. Symphynota tenuissima.—*Testâ angusto-ellipticâ, inæquilaterali, transversim rugosâ, compressâ; valvulis tenuissimis fragillimisque; margine dorsali connatâ; dente cardinali prominentiâ exiguâ, laterali unico et aciculari in valvulâ utrâque; natibus depressis; ligamento celato; margaritâ cæruleo-albâ et purpureâ, iridescente.*

Hab. Ohio. T. G. Lea. My cabinet. Cabinet of T. G. Lea. Cabinet of Prof. Vanuxem. Cabinet of P. H. Nicklin. Diam. ·6, length 2·2, breadth 2·5 inches.

8. Symphynota ochracea.—*Testâ sub-ovatâ, inæquilaterali, transversim rugosâ, inflatâ; valvulis post ligamentum connatis, tenuibus, fragilibus, et sine alâ; dentibus cardinalibus et lateralibus curvam lineam facientibus; natibus prominentibus; ligamento conspicuo; margaritâ cæruleo-albâ et ochraceâ.*

Hab. Schuylkill and Delaware. My cabinet. Cabinet of Mr. Say. Cabinet of Prof. Vanuxem. Cabinet of Mr. Hyde. Cabinet of the Academy of Natural Sciences. Cabinet of Dr. Griffith. Cabinet of P. H. Nicklin. Peale's Museum. *Unio ochraceus.* Say. Nicholson's Encyclopædia, Art. Am. Conchol. pl. 2. fig. 8. Diam. 1·3, length 1·9, breadth 2·9 inches.

9. Symphynota cygnea.—*Testâ ovatâ, antice latâ et rotundatâ, irregulariter transversim rugosâ; natibus retusis; valvulis tenuibus et post ligamentum connatis.*

Hab. rivers and lakes of Europe. My cabinet. *Mytilus cygneus.* Linn. Gmel. p. 3555. *Anodonta cygnea.* Lam.

LIX. *Pro-*

LIX. *Proceedings of Learned Societies.*

HORTICULTURAL SOCIETY.

July 6.—THE following paper was read:—An account of a new variety of plum. By Thomas A. Knight, Esq. F.R.S. &c. President.

Buds of the above plum were distributed to the Fellows.

The following matters were exhibited:—Seedling strawberries, from Joseph Lachlan, Esq.—Cucumis anguria, or snake cucumber, from Samuel Wilson, Esq.—George IV. Heartsease, from Mr. Henry Silverlock. Salvia cardinalis, a splendid new perennial, from the same. A new cos lettuce, from the same.

A very large collection of flowers and fruit was also sent from the Garden of the Society for exhibition.

July 20.—The following matters were exhibited:—A collection of carnations and picotees, from Mr. Hogg of Paddington.—Princess Augusta pelargonium, Potentilla Russelliana, and dahlia flowers, from Mr. Russell of Battersea.—Black Hamburgh grapes, from Mr. George White, gardener to Sir Rowland Hill, Bart., Hawkstone Park, near Shrewsbury.—Fruit (in spirits) of the lucuma, from Alexander Caldcleugh, Esq. of Valparaiso;—and a small gourd from the Hon. T. H. F. Strangways:—together with a large collection of flowers and fruit from the Society's Garden.

The draft of the amended Bye Laws having been read for the third time and signed by the Chairman, it was moved, That the present Bye Laws of the Horticultural Society of London be repealed. This was ballotted for, and unanimously agreed to. It was then moved, That the draft of amended Bye Laws now read for the third time be adopted. This was ballotted for, and unanimously agreed to.

August 3.—The following paper was read:— The Meteorological Journal kept in the Garden of the Society during the year 1829.

The following matters were exhibited:—Duchess of Oldenburgh apples, from Mr. Francis, Ivy House, Canterbury.—Moor Park apricots, from Mr. John George Fuller, F.H.S.—Seedling currants, from John Williams, Esq. of Pitmaston.—A large collection of flowers and fruit was also exhibited from the Garden of the Society.

August 17.—The following matters were exhibited:—Specimens of an amber-coloured grape from Portugal, Sir Abraham Pitche's black grape, Sweet water grapes and Black Prince grapes, from Charles Holford, Esq. of Hampstead.—Black Hamburgh grapes, from Miss Prest of Lewisham.—A large collection of fruit and flowers from the Garden of the Society.

September 7.—The following paper was read:—An account of a case of malformation in an exogenous tree. By John Lindley, Esq. Assist. Sec.

The following matters were exhibited:—A plant (in flower) of Camellia Chandleri, from John Allnutt, Esq. F.H.S. Flowers of Camellias, from the same.—A curious root from Pulo Penang, from J. L. Heathorn, Esq.—A collection of double dahlias, from Mr. Russell,

Russell, of Battersea.—Masulipatam melons, Guimaraen plums, from J. Biddulph, Esq.—A collection of apples, from Mr. J. Kirke. Kirke's fine plum, from the same.—Sweet red currants, from Thomas A. Knight, Esq. Two cross-bred melons, from the same.—A queen pine-apple, weighing four pounds one ounce, from Mr. William Greenshields. Tokay grapes and Muscat of Lunel grapes, from the same.—An apple unnamed, from N. W. Wickham, Esq.—Amber-coloured Portugal grapes and black grapes of Champagne, from C. Holford, Esq.—A collection of flowers and fruit from the Garden of the Society.

September 21.—The following paper was read:—Upon the state of Horticulture in Ross-shire. By Sir G. S. Mackenzie, Bart.

The following matters were exhibited:—Specimens of the Dahlia Anton, from Mr. James Sutton.—Dahlia flowers, from Mr. Chapman, gardener to the Marquess of Stafford.—Seedling apples, from the Rev. Peter Rashleigh.—A collection of flowers and fruit from the Garden of the Society.

Notice was issued from the Chair, that the Charter and new Bye Laws were now ready for delivery to the Fellows of the Society.

LX. *Intelligence and Miscellaneous Articles.*

CHARRING OF WOOD AT LOW TEMPERATURES.

MR. Charles May, chemist of Ampthill, has sent me some specimens of wood converted into nearly perfect charcoal, at a very low but long continued heat. The pieces, he informs me, are part of the bottom of a tub, which held about 130 gallons, and which had been in use in his laboratory about three years and a half, and almost constantly worked for boiling a weak solution of common salt, generally with an open steam pipe, and sometimes, though rarely, with a coil: the temperature was seldom higher than 216° or 220°, and the vessel was lined with tin rolled into sheets about 1-16th of an inch thick, and nailed to the inside; the joints, however, were not so good as to prevent the liquid from getting between the metal and the wood. Mr. May states also, that he had long since remarked, that on making extracts with steam of very moderate pressure, all the apparent effects of burning might be produced, but that he was not prepared to find so complete a carbonization of wood by steam; the vessel was made partly of fir and partly of ash, the former of which was most perfectly reduced to the state of charcoal. R. P.

LIMITS TO VAPORIZATION.

A paper on the above-named subject by Mr. Faraday was published in the Philosophical Transactions for the year 1826; when the experiments therein mentioned were published, others relating to the same subject were arranged, but which required great length of time for the development of their results. After a lapse of four years the experiments were examined, and the results are now stated.

In

In September 1826, several stoppered bottles were made perfectly clean, and several wide tubes close at one extremity so as to form smaller vessels capable of being placed within the bottles, were prepared. Then selected substances were put into the tubes, and solutions of other selected substances into the bottles: the tubes were placed in the bottles so that nothing could pass from the one substance to the other, except by way of evaporation. The stoppers were introduced, the bottles tied over carefully and put away in a dark safe cupboard, where, except for an occasional examination, they have been left for nearly four years, during which time such portion of the substances as could vaporize have been free to act and produce accumulation of their specific effects.

In this way it was found that neither sulphate of soda nor muriate of barytes were volatilized; the same was the case with solution of nitrate of silver and chloride of sodium; diluted sulphuric acid and common salt; solution of potash and arsenious acid in pieces and powder; diluted sulphuric acid and muriate of ammonia; solution of persulphate of iron and ferrocyanate of potash in crystals; solution of potash and fragments of calomel; solution of iodide of potash, and chloride of lead; solution of muriate of lime and crystals of carbonate of soda; solution of persulphate of copper and crystals of ferrocyanate of potash;—from these experiments it would appear, Mr. Faraday observes, "that there is no reason to believe that water or its vapours confer volatility, even in the slightest degree, upon those substances which alone have their limits of vaporization at temperatures above ordinary occurrence, and that consequently natural evaporation can produce no effects of this kind on the atmosphere."

From other experiments, Mr. Faraday concludes that "nitrate of ammonia, corrosive sublimate, oxalic acid, and perhaps oxalate of ammonia, are substances which evolve vapour at common temperatures."—*Journal of the Royal Institution*, October, 1830.

COMPOSITION OF GUNPOWDER.

Dr. Ure has analysed various samples of gunpowder, and the following are the results of his investigation:

	Nitre.	Charcoal.	Sulphur.	Water.	Loss.
Waltham Abbey....	74·5	14·4	10·0	1·1	
Hall, Dartford	76·2	14·0	9·0	0·5	0·3
Pigou and Wilks ...	77·4	13·5	8·5	0·6	
Curtis and Harvey..	76·7	12·5	9·0	1·1	0·7
Battle gunpowder ..	77·0	13·5	8·0	0·8	0·7

"The process," observes Dr. Ure, "most commonly practised in the analysis of gunpowder seems to be tolerably exact. The nitre is first separated by hot distilled water, evaporated and weighed. A minute loss of salt may be counted on from its known volatility with boiling water. I have evaporated always on a steam bath. It is probable that a small proportion of the lighter and looser constituent of gunpowder, the carbon, flies off in the operations of corning and dusting. Hence analysis may show a small deficit of charcoal below the

the synthetic proportions originally mixed. The residuum of charcoal and sulphur left on the double filter-paper being well dried by the heat of ordinary steam, is estimated as usual by the difference of weight of the inner and outer papers. This residuum is cleared off into a platina capsule with a tooth-brush, and digested in a dilute solution of potash at a boiling temperature. Three parts of potash are fully sufficient to dissolve out one of sulphur. When the above solution is thrown on a filter, and washed first with a very dilute solution of potash boiling hot, then with boiling water, and afterwards dried, the carbon will remain; the weight of which deducted from that of the mixed powder will show the amount of sulphur."

Dr. Ure says that he has tried other and more direct modes of estimating the sulphur, but with little satisfaction; such as dissolving it by means of hot oil of turpentine, its conversion into sulphuric acid by the use of nitric acid and chlorine, &c.

"If we inquire," says Dr. Ure, "how the *maximum* gaseous volume is to be produced from the chemical reaction of the elements of nitre on charcoal and sulphur, we shall find it to be by the generation of carbonic oxide and sulphurous acid, with the disengagement of nitrogen. This will lead us to the following proportions of these constituents:—

1 prime equivalent of	nitre......	102	75·00 per cent.
1 do. do.	sulphur....	16	11·77
3 do. do.	charcoal ..	18	13·23
		136	100·00

The [acid of the] nitre contains five primes of oxygen, of which three, combining with the three of charcoal, will furnish three of carbonic acid gas, while the remaining two will convert the one prime of sulphur into sulphurous acid gas. The single prime of nitrogen is, therefore, in this view disengaged alone.

The gaseous volume, on this supposition, evolved from 136 grains of gunpowder, equivalent in bulk to 75 grains of water, or three-tenths of a cubic inch, will be, at the atmospheric temperature, as follows:

	Grains.		Cubic Inches.
Carbonic oxide	42	=	141·6
Sulphurous acid............	32	=	47·2
Nitrogen	14	=	47·4

being an expansion of one volume into 787·3. But as the temperature of the gases at the instant of their combustive formation must be incandescent, this volume may be safely estimated at three times the above amount, or considerably upwards of two thousand times the bulk of the explosive solid."—*Ibid.*

PURPLE POWDER OF CASSIUS.

M. Buisson states that in preparing this substance, he found that the solution of gold always contains the same muriate, though it may be mixed with more or less acid; but he observes, that the solution of tin, even when well prepared, contains two different muriates, and it is upon their co-existence, within certain limits, that he conceives the

goodness of the solution to be owing. The experiments upon which this opinion is founded are the following.

1st. The solution of protomuriate of tin, as neutral as possible when mixed with a solution of gold, gives a maroon, brown, blue, green or metallic precipitate, according to its concentration and proportion, but the colour is never purple.

2nd. Pure permuriate of tin, whether acid or not, produces no change in the same solution of gold, whatever be the proportions employed.

3rd. A mixture of one part of protomuriate, nearly neutral, and two parts of permuriate of tin, with one part of muriate of gold, instantly occasions a fine purple colour. Founded on these facts, M. Buisson gives the following process for obtaining the purple powder:

4th. Dissolve about 15 grains of granulated tin in muriatic acid, either with or without heat, taking care that the solution is neutral.

5th. Prepare a solution of permuriate of tin by dissolving about 30 grains of tin in a sufficient quantity of aqua regia, composed of three parts of nitric acid and one part of muriatic acid; taking care that the solution is neutral, and free from protomuriate, which is determined by its giving no precipitate with a solution of gold.

6th. To prepare the solution of gold, dissolve about 108 grains of gold in aqua regia, composed of one part of nitric acid and six parts of muriatic acid; the solution should be nearly or quite neutral.

Dilute the solution of gold, so that a pint of it contains about 15 grains of the metal. Pour in the permuriate of tin and mix them well, then add drop by drop the protomuriate, till the required tint is produced, remembering that the protomuriate causes a brown, and the permuriate a violet colour, and intermediate proportions give a red. Wash the precipitate as quickly as possible, that no action may take place between the salts of tin and the precipitate, which alters its colour.

The purple powder of a fine tint yielded by analysis:

Metallic gold	28·5
Peroxide of tin	65·9
Chlorine	5 2
	99·6
Loss	4
	100·0

Journal de Pharmacie, October, 1830.

ANALYSIS OF VEGETABLE PRODUCTS.

The following are the results of analysis by MM. Henry, jun. and A. Plisson:

Sugar.—	Carbon	44·104
	Hydrogen	6·136
	Oxygen	49·760
		100·000

Kinic

Kinic Acid.—	Carbon	34·4320
	Hydrogen	5·5602
	Oxygen	60·0078
		100·0000

Amyrin.—The sub-resin of Elemi.

Carbon	79·11
Hydrogen	7·44
Oxygen	13·55
	100·00

Amygdalin.—A peculiar substance, discovered by MM. Robiquet and Boutron, in bitter almonds.

Carbon	0·585616
Hydrogen	0·070856
Azote	0·036288
Oxygen	0·307240
	0·1000000

Asparagin.—	Carbon	0·378175
	Hydrogen	0·056662
	Azote	0·221305
	Oxygen	0·343858
		0·1000000

Aspartic Acid.—	Carbon	0·3772
	Hydrogen	0·0537
	Azote	0·1204
	Oxygen	0·4187
		1·0000

Ibid.

ARSENIC IN SEA SALT.

The presence of arsenic in sea salt has already been observed in that found in commerce; and MM. Latour de Trie and Lefrançois, students in pharmacy, have lately detected it in a salt used in the Canton of Sézanne, in the Department de la Marne. It appears to have occasioned serious accidents; and was submitted to examination, which showed that the salt contained a quarter of a grain of deutoxide of arsenic in an ounce. The authors purchased salt in various parts of Paris, but did not detect arsenic in any one sample.—*Ibid.*

PHOSPHURET OF SULPHUR.

When the protochloride of phosphorus is exposed to the action of sulphuretted hydrogen, heat is evolved, and there is formed a solid yellow substance, without any apparent crystalline form, and adhering strongly to the glass. This is a phosphuret of sulphur. At common temperatures it decomposes water, and at length disappears in it, forming sulphuretted hydrogen and phosphoric acid. Its atomic constitution is probably

2 atoms phosphorus + 3 atoms sulphur.

Our

Our readers will remember that Faraday and Mitscherlich have described another compound, consisting of

2 atoms phosphorus + 1 atom sulphur.

Brewster's Journal.—Ann. de Chim. xliii. p. 25.

PREPARATION OF PHOSPHORUS.

Wöhler recommends, as likely to give phosphorus at a very cheap rate, to distil by a strong heat ivory-black, with half its weight of fine sand and charcoal powder. A silicate of lime is formed, and the carbonic oxide and phosphorus come over.—*Brewster's Journal.—Poggendorf.—Ann. de Phys.* xvii. p. 178.

AMMONIACAL DEUTOCHLORIDE OF TITANIUM. BY M. ROSE.

When dry ammoniacal gas is passed over deutochloride of titanium, there is strong action, accompanied with the evolution of heat, and a brownish red powder is formed. This powder combines with the portion of deutochloride which remains unacted upon, and is not further attacked by the ammoniacal gas; the mixture is to be well shaken, and when the ammonia, as detected by the smell, is in excess, the operation is finished.

When in contact with the air the compound becomes white; it liquefies in moist air. It is not perfectly soluble in water. According to analysis it consists of 84·71 of deutochloride of titanium and 15·29 of ammonia.—*Hensman's Repertoire*, vol. iii. p. 304.

ON AN ALLEGED ERROR IN THE CALCULATIONS OF THE LATE ECLIPSE OF THE MOON.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen, Bristol, Small-Street Court, Sept. 28, 1830.

I take the liberty of troubling you with the following letter, which is a copy (or nearly so) of one sent to the Literary Gazette, which the editor declined inserting. I submit it to you for insertion or rejection, as you may think fit.

The occasion of it was briefly as follows. The late eclipse of the "labouring moon" had given birth to a couple of articles in the Literary Gazette, one by C. H. Adams of Edmonton; the other by J. T. B. of Deptford, both complaining of great inaccuracy in the calculations of our almanacs in relation to that eclipse, and reflecting discredit on the science of astronomy itself. The latter gentleman says (Literary Gazette, 11th of September), after alluding to the unfavourable state of the weather on the evening of the eclipse, "The only observation made was at $10^h\ 15^m$, at which time a portion of the western limb of the moon was distinctly seen to be illuminated,—$27{\cdot}4^m$ after the time predicted for the beginning of total darkness." .... "It is, however, due to the Nautical Almanac, and other British astronomical works of a similar nature, to state that the times of the phænomena of this eclipse, as given by them, agree nearly with those in the *Connaissance des Tems* and Encke's Berlin Ephemeris, both of which are deservedly held in the highest estimation." He then quotes from the computations of the three

three Almanacs, to show how very slightly they differ from each other. I accordingly addressed the following to the Editor of the Literary Gazette; viz.

"Sir, Bristol, Sept. 18, 1830.

"The charges alleged against the accuracy of the Nautical Almanac, by your correspondents C. H. Adams of Edmonton, and J. T. B. of Deptford, are of so serious a nature, as would be sufficient, if properly substantiated, to destroy all future confidence in the calculations of that work.

"From the observations of both these gentlemen on the late eclipse of the moon, it would seem that the actual commencement of total darkness was at least half an hour later than the Almanac had predicted; and the close agreement between the calculations of our Greenwich astronomers, and those at Paris and Berlin, must indicate either that a typographical error had crept into all the copies of the astronomical tables employed at those three Observatories, or that the science of astronomy itself, even in its present advanced state, does not enable us to foretel the time of a conjunction or opposition of the sun and moon to within half an hour of the truth.

"The first of these suppositions is easily refuted, by comparing together, for a few days before and after the eclipse, the moon's longitudes, as given in the Nautical Almanac, which being computed for every noon and midnight, afford a check on each other, by forming a series of terms wherein an error, either in the tables or the calculations, amounting to a few seconds only, may be immediately detected, by the interruption occasioned in the series, when examined by first, second, &c. differences. I have in this way compared the computations of the moon's longitude for a day or two before and after the 2nd of September, and find them perfectly consistent with each other. The times of the opposition also, and of the duration of total darkness, I find to be correctly deduced from the series of the daily longitudes, &c. of the sun and moon.

"If, therefore, the assertions of these observers are to be depended upon, we are reduced to the humiliating alternative of pronouncing our best astronomical tables, with all their boasted accuracy, insufficient to determine the moon's place within a quarter of a degree: so that, had a ship's captain attempted to determine his longitude at sea by the Almanac and lunar distances on either the 1st, 2nd, or 3rd of September, he must have been thrown out in his reckoning at least four hundred geographical miles!

"Happily, however, for the credit of astronomy, and the consolation of sea-faring men, I am able to contradict the whole of these statements by my own observations, made during the eclipse; probably under a more propitious state of the atmosphere than these gentlemen were favoured with at Deptford and Edmonton. In order to verify the computations of our almanac-makers, I procured an excellent compensated watch, and set it a few hours before commencing my observations, by my friend Mr. Jones's astronomical

nomical clock in this city, which is regulated by a transit instrument, and compared the watch again on the following morning. The clouds prevented my seeing any thing of the moon until about 11 o'clock; at which time, though still in the real shadow, she was shining with comparative brightness, by the refracted light of the earth's atmosphere. Precisely at 18 minutes past 11, she began to emerge from this partial illumination into the direct rays of the sun; and allowing $10^{m}\ 20^{s}$ for the difference of longitude between Bristol and Greenwich, we find the time of the emersion at Greenwich to be $11^{h}\ 28{\cdot}3^{m}$, agreeing with the Almanac almost exactly.

"The refracted light thrown on the moon while in the earth's shadow was so strong as to surprise every beholder; and this light, no doubt, was the *ignis fatuus* which led your correspondents so far astray.

"Leaving, however, these gentlemen to explain their mistake in the best manner they can, I may safely assert, without the smallest fear of contradiction, that the predicted time of the emersion, at least, was quite correct, several other persons present agreeing with me as to that part of the phænomenon, and the same being further confirmed by two scientific gentlemen, with whom I conversed on the following day. I am, sir, your most obedient servant,

"THOS. G. BUNT."

In his "Notices to Correspondents," the editor of the Literary Gazette has acknowledged the receipt of another letter from a Mr. Smith of Newry, confirming the truth of the predictions of the Almanac in reference to the eclipse, and accounting for the mistake of Mr. Adams and J. T. B. in the same manner as I have done above.

SOME REMARKS ON THE BUSHMEN OF THE ORANGE RIVER*. BY LEWIS LESLIE, ESQ. ASSISTANT SURGEON, 45TH REGT.

In that neighbourhood †, and along the Hornberg, purer examples of that extraordinary race are perhaps nowhere to be found; and whatever follows, regards only them, and may differ from any account of other portions of the tribes along the African frontier. Small in stature as the Hottentot race is, they are, in the quarter mentioned, less than any where else, seldom exceeding five feet, but of the most perfect symmetry; they are active in their movements, but indolent in disposition; their colour is dark, but is rendered still darker by filth; their features are peculiarly forbidding, on account of the great distortion of the bones of the face; and the facial angle approaches considerably to that of the monkey. The Bushman will seldom submit to coercion and restraint,—if he does, he becomes the Boor's most wretched menial, and perhaps is worse treated than any slave in the world. In the state of liberty, they dwell in kraals, under the authority of a chief, whose rank is among them hereditary. The number in one kraal seldom exceeds thirty

* From the Edinburgh New Philosophical Journal.

† The writer refers to a military post, which was situated on a branch of the Orange River, known by the name of Nurgariep, or Black River, and close to the country inhabited by the Tambookies.

thirty—men, women, and children. Their dwellings are formed of mats, if in the plain, just large enough to creep into; but they often reside in a high and ridgy mountain, under some projecting ledge of rock, the approach to which is narrow and difficult. If attacked there, they seldom flee. They have no fear of death; and, if possessed of a more powerful weapon, might defy the attacks of the Boors, make them less frequent, and more fatal. Nothing but the privations they suffer would make any one of them submit to the cruelty of the farmers; and, living as they do on locusts, ants, and some farinaceous roots, there can be no better proof of the insufficiency of their tiny bow, and of the general inertness of their celebrated poison; yet they are themselves impressed with the conviction of its strength, and they have been able to impress their enemies with a dread of its effects, if not of its fatality. I have never been able to procure one well authenticated relation of death produced by it in man. I have known some cases of horses and dogs dying from the insertion of the arrow into the leg; but some of them seem to die rather from the effect of violent inflammation in the limb, than from any specific power in the poison itself. In one instance of a dog, however, the animal became stupid and insensible in a few minutes, and died in twenty. Some colonists who have been wounded, assert that they are subject to periodical attacks of insanity, under certain states of atmospherical influence; but I believe this to be, like most of their tales, quite unworthy of credit. The poison of the Bushman of the Hornberg is extracted from plants, and from plants only, so far as I have been able to learn. In that quarter, they use no mineral poison, nor the venom of snakes. Two specimens of plants used by them accompany this; the bulb is a species of *Hæmanthus;* but never having seen the other plant in flower, I have been unable to learn its name. Its leaf exudes a milky juice, and, cut up and bled, forms a tenacious extract, which is spread on the arrow, to some thickness. There is another plant which they use likewise, either alone or with the other two; which, together, forms the strongest they procure; its name is "mountain poison." Growing on the stony hills, and very rarely to be found, I have never got a specimen of it.

Their dexterity in the use of their bow is remarkable, and the distance they can shoot, with such a light arrow, is astonishing. They will throw the arrow upwards of a hundred yards, and with great correctness; but, as might be expected, it will seldom wound at such a distance; and I have known a cavalry cloak protect a soldier at twenty paces. The bow is not brought to the eye in shooting. They fix their eye upon the object, grasping the bow with the left hand, while the arrow passes through the fingers on the right side,—a mode of shooting I believe peculiar to them.

Their treatment of a wound made by a poisoned arrow is truly scientific. It is laid freely open, the poison cleaned out, and a horn applied in the manner of a cupping-glass, exhausted by suction at the small extremity. This, as far as I could learn, is the only treatment they adopt, never making use of any herb as a specific. The Boors

Boors consider gunpowder and urine as very efficient, and prescribe those in every arrow-wound, and in every case of snake-bite. Cupping would seem to be the Bushmen's favourite treatment of every complaint accompanied with pain, and so frequently do they resort to this, that by the time they are full grown they appear scars all over.

The length of time a Bushman can live without food is surprising, often living for three and four days without a mouthful; and the quantity they can devour after such abstinence is equally remarkable, one man having been known to eat an African sheep (thirty pounds) in a single night. When unable to procure food, a belt round the body is tightened as the craving increases, and they resort to the smoking of *dakka* (a species of chanvre or hemp), which produces intoxication. The narcotic effects of this plant no doubt produce much of that shrivelled appearance which is observable in all of any age. When possessing plenty of their *dakka*, they can smoke and sleep for several days and nights without eating.

A Bushman has no idea of the perpetuation of property; I might say, no notions of a prospective existence. He is wholly dependent on nature or on man: he will neither imitate the Caffer nor the Boor, will neither grow corn nor breed cattle.

The figures drawn by them on the rocks are often remarkable for the correctness of the outline; they hit the attitude of the animal, but seldom care about truth in the colouring: speaking phrenologically, they have the organ of form, but not of colour. I have never seen any animal resembling the unicorn among their paintings, but such an animal is said to exist beyond the Orange River. They are fond of music and dancing, but their musical instrument is rude, and without power or variety, consisting of one string stretched upon a bow, whose vibrations are produced by the breath, with great exertion.

The Bushman's conception of a Supreme Being is, that he is an evil deity; and their notion of futurity, that there will be an eternity of darkness, in which they will live for ever, and feed on grass alone. They imagine that the sun sends rain, and when he is clouded, they hold up burning wood, in token of disapprobation. They believe that the sun and moon will disappear, to produce the darkness they anticipate.

The Bushman's bow is made of a peculiar tree, called the Blue Bush, whose branches are almost moulded by nature to the artificial form. The sinews of the quagga yield powerful bow-strings, and the arrow is formed of a slender reed, headed with antelope's horn, and pointed with a small triangular piece of metal, which they procure from the Caffers.

AURORA BOREALIS.

In the evening of the 7th this interesting meteoric phænomenon was observed at Gosport, from fifteen minutes before till a quarter past nine o'clock, when the moon rose, and her light overpowered it. The

The aurora was rather faint, and it only extended from the N. to the N.W. point of the horizon, yet there rose from its base about twelve columns of light with other coruscations; and one meteor appeared over it. There was also a faint appearance of the aurora the following evening, but large black clouds intervened. In the evening of the 17th the Northern Lights again appeared, from eight till a quarter past nine o'clock: during the first half-hour, many bright flame-coloured columns rose in slow succession, some of which attained the same altitude as the star Benetrasch in Ursa Major, and were two degrees in width. Four meteors occasionally appeared over them. The aurora then settled into a segment of mild light, and extended from N. by E. to W. The heavy dew and low passing cirrostratus clouds were unfavourable to its continuance.

ASTRONOMICAL OBSERVATIONS.

The eclipse of the moon in the night of the 2nd of September was visible here only in its effects, in consequence of the interposition of thick strata of clouds, through which only an undefined light was seen. At a quarter past nine, however, a partial opening in the clouds showed for a few seconds of time, by means of a telescope, that the moon was nearly half eclipsed.

The planet Mars, on fine evenings this month, has been a very conspicuous and interesting object in the S.E. quarter of the heavens, having come to an opposition with the sun at midnight of the 19th; and from his comparative nearness to the earth, has on several evenings, before and after his opposition, appeared to surpass Jupiter in splendour and magnitude: he will continue to be conspicuous in the evenings during the remainder of the year, but will gradually decrease in brilliancy and apparent magnitude, and lose his circular form as he passes to a conjunction with the sun.

Mercury, at his greatest elongation in the evenings of the 17th and 18th, was scarcely perceptible with the naked eye, and had but a faint appearance through a telescope, although the sky was clear, owing to his position in south declination.

Georgium Sidus or Herschel planet, is creeping along the neck of the Goat, and is now (October 1st) on the meridian one minute and twenty seconds after α Cygni, and 34 degrees distant from, and immediately under, α Delphini, at 8 o'clock in the evening; so that good opportunities may occur during the month for observing it with a powerful telescope.

Emersions of the first and second Satellites of Jupiter.—In the evening of the 24th, at $8^h\ 41^m\ 52^s$ mean time, an emersion of the first satellite from Jupiter's shadow, on the east side of the planet, was observed here with an achromatic telescope, and the difference in time compared with that in the Nautical Almanac, gave the longitude from Greenwich within a mile and a half.

In the evening of the 27th, at $8^h\ 18^m\ 3^s$ mean time, an emersion of the second satellite out of Jupiter's shadow was observed with

 the

the same telescope, which, compared with Greenwich time of the emersion, gave the longitude within one mile, notwithstanding the nearness of the moon.

LUNAR OCCULTATIONS.

List of Occultations of fixed Stars by the Moon in November and December 1830. *Computed for Greenwich, by* THOMAS HENDERSON, *Esq.; and circulated by the Astronomical Society.*

1830.	Stars' Names.	Mag.	Ast. Soc. Cat. No.	Immersions.				Emersions.			
				Sidereal time.	Mean solar time.	Angle from North Pole.	Angle from Vertex.	Sidereal time.	Mean solar time.	Angle from North Pole.	Angle from Vertex.
				h m	h m	°	°	h m	h m	°	°
Nov. 5	r Cancri	6	981	3 32	12 33	69	29	4 35	13 36	283	246
26	f Piscium	6	139	22 36	6 15	82	55	23 34	7 13	328	310
29	48 Tauri	6	468	22 25	5 53	110	70	23 19	6 46	280	240
—	γ ——	3·4	478	0 4	7 32	106	66	1 7	8 34	286	252
—	75 ——	6	508	4 34	12 0	147	152	5 23	12 50	237	253
—	θ^1 ——	5	510	4 26	11 53	59	62	5 18	12 45	325	340
—	θ^2 ——	5·6	511	4 53	12 20	12	21	☽ almost touching star; occulted to places further north.			
—	(99) ——	5·6	516	5 23	12 49	89	106	6 31	13 57	291	319
—	Aldebaran	1	528	8 2	15 28	117	155	8 59	16 25	256	296
30	111 Tauri	6	640	0 55	8 18	43	3	1 25	8 49	343	304
—	115 ——	5·6	651	2 15	9 38	151	115	2 58	10 21	231	200
—	N ——	6	706	11 49	19 10	22	62	12 9	19 30	336	15
Dec. 2	f_1 Gemin.	6	940	6 19	13 33	147	128	6 54	14 8	203	192
9	l^9 Virginis	6	1545	Under horizon.				8 22	15 9	256	218
22	q Piscium	5	2869	21 37	3 35	101	78	22 48	4 45	307	294
25	μ Ceti	4	293	21 35	3 21	99	60	22 33	4 18	301	264
28	N Tauri	6	706	22 25	3 58	63	26	23 5	4 39	313	274

The *angles* are reckoned from the northernmost point, and also from the vertex, of the moon's limb, towards the right hand, round the circumference of the disc, as exhibited in an inverting telescope.

SCIENTIFIC BOOKS.

Preparing for Publication.

On the Proceedings of the Royal Society, as connected with the Decline of Science in England; together with Arguments proving that before the Society can regain confidence at home, or respect from abroad, a reform of its conduct and a re-modelling of its Charter are indispensable. By Sir James South, Fellow of the Society, and late Member of its Council.

LIST

LIST OF NEW PATENTS.

To T. Hancock, Goswell Mews, Goswell Road, Middlesex, water-proof cloth manufacturer, for improvements in the manufacture of certain articles of dress or wearing apparel, fancy ornaments and figures; and in the method of rendering certain manufactures and substances, in a degree or entirely, impervious to air and water; and of protecting certain manufactures and substances from being injured by air, water, or moisture.—Dated the 5th of August, 1830.—2 months.

To W. Mallet, Marlborough-street, Dublin, iron manufacturer, for certain improvements in making or constructing certain descriptions of wheel-barrows —5th of August.—6 months.

To J. Pearse, Tavistock, Devon, ironmonger, for an improved method of making and constructing wheels, and in the application thereof to carriages.—5th of August.—6 months.

To C. Shiels, Liverpool, merchant, for certain improvements in the process of preparing and cleansing rice. Communicated by a foreigner.—5th of August.—6 months.

To Æ. Coffey, Dock Distillery, Dublin, distiller, for certain improvements in the machinery used in the process of brewing and distilling.—5th of August.—6 months.

To M Robinson, Great George-street, Westminster, navy agent, for certain improvements in the process of making and purifying sugars. Communicated by a person residing abroad.—5th of August.—6 months.

To R. Clough, Liverpool, ship-broker, for an improved supporting block, to be used in graving docks, and for other purposes.—5th of August.—6 months.

To Sir C. W. Dance, Hertsbourne Manor Place, in the parish of Bushy, Hertfordshire, knight, lieut.-colonel, for certain improvements in packing and transporting goods.—5th of August.—6 months.

To S Smith, Princes-street, Leicester Fields, in the parish of St. James, Westminster, gun-maker, for a new nipple or touch-hole, to be applied to fire-arms for the purpose of firing the same by percussion; and a new cap or primer for containing the priming, by which such fire-arms are to be fired.—7th of August.—2 months.

To W. Palmer, Wilson-street, Finsbury-square, gentleman, for improvements in making candles.—10th of August.—6 months.

To J. Lawrence, Birmingham, silversmith, and W. Rudder, Edge, Gloucestershire, gentleman, for an improvement in saddles and girths by an apparatus affixed to either of them.—10th of August.—6 months.

To T. Ford, Canonbury-square, Islington, Middlesex, chemist, (nephew and successor to the late R Ford,) for certain improvements in the medicine for the cure of coughs, colds, asthmas, and consumptions; known by the name of "Ford's Balsam of Horehound."—12th of August.—2 months.

To J. Knowles, Farnham, Surrey, hop-planter, for a certain instrument or machine for drawing up hop-poles out of the ground, previous to

to picking the hops; and which, by drawing the poles perpendicularly, will greatly save them, as well as prevent the hops from being bruised, called "a hop-pole drawer by lever and fulcrum."—13th of August.—2 months.

To M. Towgood, Dartford, Kent, paper-maker, and L. Smith, Paternoster Row, stationer, for an improved mode of applying size to paper.—18th of August.—6 months.

To Major-Gen. J. Gubbins, Southampton, for certain improvements in propelling and giving motion to machinery.—18th of August.—6 months.

To S. R. Bakewell, Whiskin-street, in the parish of St. James, Clerkenwell, Middlesex, brick and stone-ware manufacturer, for certain improvements in machinery, apparatus, or implements to be used in the manufacture of bricks, tiles and other articles to be formed or made of clay, or other plastic materials; part of which machinery is also applicable to other useful purposes. Partly communicated by a foreigner.—18th of August.—6 months.

To W. Mason, Margaret Street, Cavendish Square, axletree-maker, for certain improvements in axletrees, and also the boxes applicable thereto.—24th of August.—6 months.

To T. Barratt, St. Mary Cray, Kent, paper-maker, for certain improvements in machinery for making paper.—31st of August.—6 months.

To A. Applegath, Crayford, Kent, printer, for certain improvements in printing-machines.—31st of August.—6 months.

To W. Losh, Esq. of Benton House, Northumberland, for certain improvements in the construction of wheels for carriages to be used on railways.—August 31st.—6 months.

To E. Budding, of the Thrupp, in the parish of Stroud, Gloucestershire, machinist, for a new combination and application of machinery for the purpose of cropping or shearing the vegetable surface of lawns, grass plats, and pleasure-grounds, constituting a machine which may be used with advantage, instead of a scythe for that purpose.—31st of August.—2 months.

To J. Hanson, Huddersfield, Yorkshire, plumber and brazier, for certain improvements on locomotive carriages.—31st of August.—6 months.

To E .Clayton, of Bridlesmith Gate, Nottingham, baker, for an improved mode of manufacturing dough or paste for the purpose of baking into bread.—31st of August.—2 months.

To T. Thacher, Birmingham, Warwickshire, saddler, for an elastic self-adapting saddle.—7th Sept.—6 months.

To P. Williams, Holywell, Flintshire, surgeon, for an apparatus or contrivance for preventing accidents in carriages, gigs, and other vehicles, instantly and effectually liberating horses or other animals from the same, when in danger, or otherwise; and for locking and securing the wheels thereof in cases of danger, emergency, or otherwise.—7th Sept.—6 months.

To C. B. Vignoles, Furnival's Inn, London, and J. Ericsson, Brook Street, Fitzroy-squure, civil engineer, for certain additions to the

engines

engines commonly called locomotive engines.—7th of September.—6 months.

To W. Cook, Redcross-square, Cripplegate, London, tinworker, for certain improvements on cocks for supplying kitchen ranges and cooking apparatus with water, and for other purposes, to be called "fountain cocks."—7th Sept.—6 months.

To H. G. Pearce, Liverpool, Lancashire, master-mariner, R. Gardner and J. Gardner, of the same place, merchants, for an improved fid.—7th Sept.—6 months.

To J. Chadley, Gloucester-street, Queen-square, surveyor, for certain improvements in making or forming bricks, tiles, and chimney bars, applicable to the building or erecting the flues of chimneys.—13th Sept.—6 months.

To S. Smith, Wilton-crescent, St. George's parish, Hanover-square, builder, for certain improvements in chimneys for dwelling-houses and other buildings.—14th Sept.—2 months.

METEOROLOGICAL OBSERVATIONS FOR SEPTEMBER 1830.

Gosport:—Numerical Results for the Month.

Barom. Max. 30·40. Sept. 27. Wind S.W.—Min. 29·25. Sept. 21. Wind W.
Range of the mercury 1·15.
Mean barometrical pressure for the month .... 29·821
Spaces described by the rising and falling of the mercury............ 6·960
Greatest variation in 24 hours 0·460.—Number of changes 24.
Therm. Max. 66°. Sept. 3. Wind W.—Min. 42°. Sept. 29. Wind N.
Range 24°.—Mean temp. of exter. air 56°·05. For 31 days with ☉ in ♍ 57·32
Max. var. in 24 hours 18°·00.—Mean temp. of spring-water at 8 A.M. 53·32

De Luc's *Whalebone Hygrometer.*

Greatest humidity of the atmosphere, in the evening of the 9th...... 98°
Greatest dryness of the atmosphere, in the aftern. of the 5th & 11th 50·0
Range of the index .... 48·0
Mean at 2 P.M. 57°·4.—Mean at 8 A.M. 67°·9.—Mean at 8 P.M. 73·5
—— of three observations each day at 8, 2, and 8 o'clock .... 66·3
Evaporation for the month 2·40 inches.
Rain in the pluviameter near the ground 2·80 inches.
Prevailing wind, S.W.

Summary of the Weather.

A clear sky, 3½; fine, with various modifications of clouds, 16½; an overcast sky without rain, 4½; rain, 5½.—Total 30 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
24	12	30	1	27	27	22

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
2½	½	0	0	2	10	7½	7½	30

General

General Observations.—This month has been generally wet and windy, but most of the rain fell in the night-time. From the 8th to the 24th it rained every day more or less, frequently accompanied with boisterous equinoctial gales.

Hoar frost appeared in the grass-fields early in the mornings of the 22nd and 26th. At 11 A.M. on the 28th, a greater number of swallows congregated over Gosport and its neighbourhood than has been seen for many years; perhaps there were not less than 1200 or 1500 at one view, flying leisurely in all directions, and intermixing with each other: after making a few circular flights for ten minutes, they rose three or four hundred feet in height, and then departed in a southerly direction, except a few young ones; making their stay here twenty-three weeks and two days.

The mean temperature of the external air this month is four degrees under the mean of September for many years past.

The atmospheric and meteoric phænomena that have come within our observation this month, are one anthelion, one parhelion, twenty-six meteors, four rainbows, three auroræ boreales, lightning in the evening of the 20th, and thirteen gales of wind, or days on which they have prevailed, namely, one from the South, eight from the South-west, two from the West, and two from the North-west.

REMARKS.

London. — September 1, 2. Fine. 3. Fine: rain in the afternoon. 4. Fine: rain at night. 5. Rain. 6. Heavy rain, with thunder in the afternoon. 7, 8. Cloudy. 9. Foggy: rain at night. 10. Showery. 11. Fine: rain at night. 12. Heavy rain. 13. Fine: rain at night. 14. Fine: thunder-showers in the afternoon. 15. Heavy showers, with brisk wind. 16. Fine. 17, 18. Rainy. 19. Windy, with slight showers. 20. Fine. 21. Heavy rain. 22. Very fine. 23. Showery. 24. Fine in the morning: stormy and wet in the afternoon. 25. Windy and cold. 26. 27. Fine. 28, 29. Cloudy. 30. Foggy.

Penzance.—September 1—3. Clear. 4. Clear: rain at night. 5, 6. Fair. 7, 8. Clear. 9. Rain: fair. 10, 11. Fair: rain. 12—14. Fair: showers. 15. Rain. 16. Clear: showers. 17. Showers. 18. Clear. 19. Rain. 20. Fair: rain. 21. Showers. 22. Fair: rain. 23, 24. Showers. 25—28. Fair. 29. Clear. 30. Fair.

Boston.—September 1. Cloudy. 2. Cloudy: total eclipse of the moon; cloudy great part of the time. 3. Cloudy. 4. Fine. 5. Cloudy: rain early A.M. 6. Fine: rain early A.M. and rain at night. 7. Cloudy. 8. Cloudy: rain A.M. 9. Cloudy. 10. Rain: rain early A.M. 11. Fine: rain A.M. 12. Fine: rain early A.M. 13, 14. Cloudy: rain early A.M. 15. Fine. 16. Fine: heavy rain early A.M. 17. Cloudy. 18. Cloudy: rain A.M. 19. Cloudy. 20. Stormy: heavy rain early A.M. 21. Rain. 22. Fine. 23. Stormy: rain early A.M. 24. Fine: rain early A.M.: rain P.M. 25. Stormy. 26, 27. Cloudy. 28. Cloudy: rain at night. 29. Cloudy: rain early A.M. 30. Fine.

Meteoro-

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1830.	Barometer. London. Max.	Barometer. London. Min.	Barometer. Penzance. Max.	Barometer. Penzance. Min.	Barometer. Gosport. Max.	Barometer. Gosport. Min.	Barometer. Boston 8½ A.M.	Thermometer. London. Max.	Thermometer. London. Min.	Thermometer. Penzance. Max.	Thermometer. Penzance. Min.	Thermometer. Gosport. Max.	Thermometer. Gosport. Min.	Thermometer. Bost. 8½ A.M.	Wind. Lond.	Wind. Penz.	Wind. Gosp.	Wind. Bost.	Evap. Gosp.	Rain. Lond.	Rain. Penz.	Rain. Gosp.	Rain. Bost.
Sept. 1	30·326	30·256	30·30	30·30	30·36	30·30	29·70	75	42	64	49	65	50	58	SW.	N.	NW.	W.	...	...	...	...	...
○ 2	30·212	29·936	30·20	30·05	30·24	30·04	29·48	70	51	63	49	65	53	57	SW.	SW.	SW.	W.	...	0·01	...	0·010	...
3	29·931	29·809	30·00	30·00	29·90	29·89	29·13	66	51	61	53	66	50	57	W.	NW.	NW.	W.	0·30	·07	...	...	...
4	29·956	29·858	30·00	29·95	29·99	29·96	29·36	70	49	61	51	66	55	55	W.	N.	W.	NW.	...	·17	...	·110	...
5	29·703	29·696	29·85	29·80	29·75	29·75	29·07	68	50	62	51	64	53	56·5	W.	N.	NW.	NW.	...	·01	0·230	...	0·13
6	29·570	29·511	29·80	29·80	29·64	29·59	28·90	64	52	62	54	63	52	56·5	W.	N.	W.	NW.	·30	·45	...	·105	·08
7	29·889	29·745	29·90	29·85	29·88	29·75	29·07	65	52	61	53	62	51	58	NW.	NW.	NW.	NW.	...	·05	...	·030	·14
8	30·025	30·010	30·00	29·96	30·05	29·98	29·41	66	42	63	52	65	51	58	SW.	W.	N.	NE.	...	...	...	...	·19
9	29·858	29·698	29·75	29·75	29·97	29·69	29·25	60	52	62	52	60	50	53·5	S.	NW.	S.	NE.	·20	·31	·500	·140	...
10	29·747	29·663	29·80	29·75	29·75	29·67	29·02	65	42	57	48	61	45	56	W.	NW.	W.	NW.	...	·10	·070	·050	·43
11	29·790	29·692	29·90	29·60	29·80	29·75	29·20	66	48	59	48	61	52	53·5	S.	W.	W.	W.	...	·12	·400	·275	·18
12	29·393	29·340	29·50	29·40	29·42	29·35	28·80	64	41	59	51	61	48	54	S.	W.	SW.	W.	·25	·26	·155	·140	·38
13	29·538	29·504	29·60	29·50	29·60	29·55	28·95	67	44	60	48	62	52	49	W.	W.	NW.	NW.	...	·06	·170	·060	·16
14	29·536	29·441	29·45	29·45	29·56	29·46	28·83	68	44	58	48	61	51	54	SW.	SW.	W.	W.	...	·01	·055	·055	·28
15	29·669	29·613	29·50	29·40	29·67	29·61	29·07	64	55	62	52	65	55	56	SW.	SW.	SW.	W.	·25	·18	·900	·190	...
16	29·808	29·609	29·60	29·60	29·78	29·63	28·95	67	43	60	52	63	53	56·5	W.	W.	SW.	W.	...	·06	·090	·150	·53
17	29·702	29·602	29·65	29·60	29·67	29·61	29·22	62	46	61	50	62	51	52	NW.	SW.	NE.	calm	...	·21	·400	·190	...
18	29·889	29·587	29·80	29·80	29·90	29·55	29·06	67	40	62	49	63	49	52	SW.	NW.	NW.	calm	·20	·07	...	·040	...
19	29·962	29·667	29·85	29·60	29·97	29·73	29·33	63	50	60	52	62	53	51·5	W.	SW.	SW.	W.	...	·10	·790	·360	·07
20	29·641	29·496	29·70	29·35	29·66	29·55	28·96	66	42	58	49	59	52	56	SW.	W.	SW.	W.	...	·45	·320	·380	·08
21	29·367	29·211	29·30	29·25	29·35	29·25	28·70	61	32	58	45	58	45	53	S.	W.	W.	SE.	·20	·10	·400	·020	·22
22	29·695	29·579	29·70	29·60	29·73	29·56	29·02	66	48	59	48	59	50	50	SW.	NW.	W.	W.	...	·18	·990	·360	·12
23	29·667	29·326	29·75	29·40	29·70	29·43	28·82	66	45	58	52	62	50	53	SW.	NW.	SW.	W.	...	·01	·140	·010	·14
24	29·679	29·597	29·80	29·70	29·70	29·63	29·10	62	46	56	47	59	49	52	SW.	NW.	SW.	W.	·30	·08	·320	·035	·05
25	29·974	29·750	30·00	29·95	29·98	29·84	29·05	62	40	59	49	60	44	53·5	SW.	NW.	NW.	W.	...	...	...	...	·05
26	30·306	30·214	30·15	30·15	30·35	30·21	29·60	65	38	61	50	62	49	53	S.	W.	W.	NW.	...	...	...	...	...
27	30·411	30·367	30·20	30·20	30·40	30·39	29·79	63	51	63	54	63	54	53·5	W.	SW.	SW.	W.	·20	·15	...	...	...
28	30·283	30·117	30·20	30·10	30·30	30·15	29·66	60	48	60	54	65	48	55	S.	NW.	SW.	W.	...	...	·100	·090	—
29	30·073	30·024	30·08	30·08	30·08	30·06	29·47	61	38	56	49	60	42	51	SW.	N.	N.	W.	...	...	...	...	·97
30	30·161	30·076	30·05	30·00	30·08	30·08	29·55	63	35	56	48	59	43	49	W.	NW.	N.	W.	·20	—	...	...	...
	30·411	29·211	30·30	29·25	30·40	29·25	29·18	75	32	64	45	66	42	54					2·40	3·11	6·030	2·800	4·20

Calendar of the Meetings of the Scientific Bodies of London for 1830–31.

Societies.	Time of Meeting.	November.	December.	January.	February.	March.	April.	May.	June.
Royal .... Somerset-House.	Thursday, 8½ P.M.	18, 25, 30*	9, 16, 23	13, 20, 27	3, 10, 17, 24	3, 10, 17, 24	14, 21, 28	5, 12, 19	2, 9, 16
Antiquaries ... Somerset-House.	Thursday, 8 P.M.	18, 25	2, 9, 16, 23	13, 20, 27	3, 10, 17, 24	3, 10, 17, 24	14, 23*, 28	5, 12, 19	2, 9, 16
Linnean ...... Soho-Square.	Tuesday, 8 P.M.	2, 16	7, 21	18	1, 15	1, 15	5, 19	3, 24*	7, 21
Horticultural Regent-Street.	Tuesday, 1 P.M.	2, 16	7, 21	4, 18	1, 15	1, 15	5, 19	2*, 3, 17	7, 21
Society of Arts Adelphi.	Wednesd. 7½ P.M.	3, 10, 17, 24	1, 8, 15, 22	12, 19, 26	2, 9, 16, 23	2, 9, 16, 23, 30	6, 13, 20, 27	4, 11, 18, 25	1, 8
Royal Society of Literature Parliament-St.	Wednesd. 3 P.M.	3, 17	1, 15	5, 19	2, 16	2, 16	6, 20, 28*	4, 18	1, 15
Gelogical ...... Somerset-House.	Wednesd. 8½ P.M.	3, 17	1, 15	5, 19	2, 16, 18*	2, 16, 30	13, 27	11, 25	8, 22
Zoological Society..... Bruton-Street.	Thursday, 3 P.M.	4	2	6	3	3	7, 29*	5	2
Astronomical Lincoln's-Inn Fds.	Friday, 8 P.M.	12	10	14	11*	11	8	13	10
Royal Institut. Albemarle-St.	Friday, 8½ P.M.	...	...	21, 28	4, 11, 18, 25	4, 11, 18, 25	15, 22, 29	2*, 6, 13, 20, 27	3, 10
Royal Asiatic Grafton-Street.	Saturday, 2 P.M.	...	4, 18	1, 15	5, 19	5, 19	16	7	7*, 18 July 2, 16

* ANNIVERSARIES.—Royal Society, Nov. 30, 11 A.M.—Astronomical, Feb. 11, 3 P.M.—Geological, Feb. 18, 1 P.M.—Antiquaries, April 23, 2 P.M.—Royal Society of Literature, April 28.—Zoological Society, April 29, 1 P.M.—Royal Institution, May 2.—Horticultural Society, May 2.—Linnean, May 24, 1 P.M.—Asiatic, June 7, 1 P.M.

[Copies of the Calendar on Cards may be had at the Office of the Philosophical Magazine and Annals, Red Lion Court, Fleet Street.]

THE

PHILOSOPHICAL MAGAZINE

AND

ANNALS OF PHILOSOPHY.

[NEW SERIES.]

DECEMBER 1830.

LXI. *An Examination of those Phænomena of Geology, which seem to bear most directly on theoretical Speculations. By the Rev.* W. D. Conybeare, *M.A. F.R.S. F.G.S. &c.*

[In Continuation from p. 362.]

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

II.* FROM the organic remains included, &c. we are sure that far the largest proportion (say $\frac{9}{10}$ths) of the stratified rocks (viz. all from the lowest transition strata upwards, now forming the surface of our continents,) were deposited gradually and slowly through a long series of time beneath the sea; and *à fortiori*, the inferior rocks must have been so situated: *ergo*, originally the whole mass of our continents was beneath the sea.

III. Interposed among and breaking through these strata are certain intrusive and unstratified masses, which from the phænomena connected with them are now generally allowed

* I here shortly recapitulate the principal phænomena of the subject, however obvious and little disputed, as it may be convenient in the course of the ensuing argument frequently to refer to them: and for the purposes of that reference the numerical arrangement seems convenient.

I have also to correct an erratum in my last, p. 361, where I must have written Atlas instead of Typhon;—the classical reader will pardon the confusion when he recollects that, besides the celebrated passage in Pindar, a picturesque description of "this overwhelmed and inefficient bulk pressed beneath the roots of Ætna," occurs in a speech in the Prometheus of Æschylus intermingled with the fortunes of Atlas.—While on classical subjects, I would just remark how much I am gratified by finding every quotation in Mr. Lyell's able remarks on the attention of the ancients to geology, identical with those previously given in my own Outlines, with the single exception of the passage from Strabo, to which, however, I have given a reference although certainly partial and imperfect: as there is not a word of acknowledgement, of course this coincidence is accidental.

 to

to have been of ancient volcanic origin: the greenstone and other trap rocks are now almost universally thus considered; and the granitic rocks have so many points of analogy with these, that they can scarcely be referred to a different origin.

Observation.—It would be a useless expense of space here to enter into the detail of the phænomena thus referred to, as they are universally familiar, and the inferences from them generally allowed.

IV. The disturbances which the strata appear to have suffered subsequently to their original formation (as shown by faults, the inclined and contorted positions of beds which must have been deposited horizontally, &c.) prove that they must have undergone very extensive mechanical dislocation.

Observation.—Of course it is natural to look to this mechanical dislocation as the proximate cause of the elevation of the continents, by which they have emerged from their original subaqueous position (sect. I.); and further, we seem in the volcanic agency indicated in No. II., to find a cause adequate to produce this dislocation. Of course we must conceive this volcanic agency to have affected the surface with greater or less violence at different periods, in proportion as we find the strata more or less dislocated at those periods.

V. In all the countries hitherto examined, we find these dislocations affecting the oldest formations in the most violent manner, and to a degree beyond all comparison greater than those of newer deposition. Thus we find the transition formations universally and most violently disturbed; the carboniferous series very generally and very greatly so; the new red sandstone and all the superior rocks in most countries comparatively very little deranged;—although faults, &c. still occur, and prove that the same disturbing causes were still in action, though with an energy much diminished. In particular localities however, (but these of very limited extent compared with the entire surfaces of our continents, as far, that is, as hitherto explored,) these disturbances extend even to the tertiary formations. It generally* appears that these localities are in the neighbourhood of the most lofty mountain chains: it seems indeed established, that the height of these chains bears a general proportion to the geological period during which

* I say 'generally,' because our own Southern coast in the Isle of Wight and Dorset, presents an example of exception, namely, of violent convulsions having affected the chalk and lowest tertiary beds without producing any mountain chain. I stated originally the above law as to the height of mountain chains, with relation to the periods of disturbance, in the Number of the Annals for January 1823, pp. 3 and 4, exemplifying it with reference to the Alps and Pyrenees. I believe the idea was at that time original; it seems since to have occurred also to Elie de Beaumont.

the

the strata of the vicinity appear to have been subjected to the violent action of disturbing forces. Lastly, In the actual geological period, including the whole time which has elapsed since the continents assumed their present general form, the violent action of the disturbing forces has been confined to the districts of active volcanic vents; districts, however, extensive if abstractedly considered, yet bearing no conceivable proportion to the universal prevalence of these forces in the earliest formations: in the same manner as the utmost convulsions actually produced, though often considerable if viewed in this isolated manner, yet sink into perfect insignificance if compared with the ancient disturbances effected in our strata.

Observations.—I would now examine rather more in detail the principles thus generally enounced; in doing which, I shall be enabled to point out both what may be considered as already ascertained on the only sure foundation of patient observation, and to indicate how much further those observations must be extended, and in what directions they must be prosecuted, before we are in possession of sufficient data for any thing like a complete Theory on all the points considered in this article.

First, then, I would state the general principle of the investigation. Suppose any number of stratified masses *a*, *b*, *c*, *d* superimposed upon each other in the above order (*a* being the lowest), and disturbed by dislocating forces. Suppose them all to indicate the effects of these forces.

It is evident that this may result from two different modes of operation of those forces: for either, 1st, they may have acted frequently during the formation of each of the affected strata; or, 2ndly, they may have acted once only in a single general convulsion subsequently to the formation of all the strata. Now it is obvious, that in order to authorize any speculation as to the prevalence of the disturbing forces at particular geological periods, we must first endeavour to ascertain which of these cases has taken place. The only method by which we can accomplish this, is to ascertain whether the strata be conformable or unconformable; for it is clear that the strata which are conformable can alone be supposed to have been affected by a single convulsion: for instance, a shock affecting *a*, *b*, *c*, *d* at the same time would probably incline them all in the same direction; but, on the contrary, if a shock affected *a*, before the formation of *b*, &c., it is clear that the latter could not conform to the dislocations produced by that shock.

To ascertain the conformity or non-conformity of the strata

is, therefore, obviously of the very first importance towards enabling us to theorize on the æras of the geological disturbances. With regard to the greater aggregates constituting formations, this investigation has been prosecuted to a certain extent; but very much remains to be done even in this department: and in order to enable us to push the inquiry to its full extent, not only aggregate formations but individual beds should be similarly examined in every case of disturbed strata. We ought in this manner to ascertain the exact limits, and thence to infer the geological æra of the disturbance;—but in what instances has this yet been done? Enough, however, has I conceive been accomplished to authorize the general conclusions enounced in the above article. The disturbances of the transition formations appear to be universal; they are well exhibited in the coasts of Devonshire and Cornwall, and St. Abbe's Head, Berwickshire. I have, indeed, never seen these rocks exhibited in a single locality, or read any description of such, where they did not appear to have suffered the most violent derangements.

The period of a great part of these disturbances appears to have been antecedent to the formation of the carboniferous formations (although of course the transition rocks must have been affected by the subsequent convulsions which acted on the strata also); for we find a want of conformity, and the planes of the transition rocks appear to have been greatly contorted before the application of the planes of the carboniferous series of strata upon them. There is, indeed, some degree of difficulty in making the requisite observations, because the graywacké rocks have generally a false cleavage, not parallel to the true lines of stratification, which can only be observed by tracing the junction of heterogeneous beds, *e. g.* graywacké and transition lime, &c. In this manner the non-conformity of the transition rocks of the English lake district, and the superincumbent carboniferous strata of the Cross-fell chain, has been observed. The same non-conformity of the South-Welsh carboniferous and transition strata may be observed along the north edge of the coal-field.

In Somersetshire the junction is altogether connected by the overlying new red sandstone. The Scotch coal-fields have not yet been examined with this view; nor, as far as I am aware, any of the continental depôts. In Ireland the carboniferous rocks of the Connaught and Leinster coal districts rest unconformably on the older formations; and a similar position is often noticed in Weaver's excellent Memoir on the South-east of Ireland.

The

The carboniferous strata are very much and very generally disturbed, but yet considerably less so than the subjacent transition formations. Thus the strata of the Cross-fell chain and the Durham coal-field seldom deviate by more then ten degrees from the horizontal line, although often traversed by very considerable faults, sometimes effecting dislocations of nearly 1000 feet. The same remarks may be applied to the Derbyshire coal-field. The South-Welsh coal-field is very unequally disturbed: nothing can exceed the derangements of the Pembrokeshire portion (see the sections in De la Beche's memoir, Geol. Trans. second series, vol. i. In Glamorganshire, the northern portion of the coal tract generally presents but a low angle of inclination (about 12°): but here occur several considerable faults, in connection with which the strata become vertical and much contorted. In the Southern portion the disturbance is more general, and the angle varies from 45° to 70°.

In the Somersetshire coal-field the disturbance is very considerable, especially towards the Mendip chain, where the limestone is often nearly vertical, and the superincumbent coal beds thrown quite over, and doubled into zigzags. Near Clevedon also a large tract of limestone covered with coal-measures appears to have subsided to such an extent, that near the line of fracture the subsided strata crop out three miles to the west of the main chain with which they must originally have been continuous.

On the Continent, the coal-measures about Valenciennes and throughout the district of the Meuse exhibit the most remarkable contortions and derangement (see Oeynhausen and Decken's excellent *Bemerkungen*): such also appears to be generally the case in Germany (see H. de Villefosse, *Richesse Minérale de la Westphalie*).

These convulsions of the carboniferous beds are clearly all referable to one single geological period;—that, namely, of the formation of these deposits; for they do not extend * to the next superincumbent formations of magnesian limestone and new red sandstone, which, on the contrary, repose on them in beds nearly horizontal; so that an eminent geologist, Omalius d'Halloy, assumes as the general division of the rocks of which he treats; 1st, inclined formations; viz. transition and carboniferous: and, 2ndly, horizontal, including all the superstrata. Nor, in fact, can we consider the principal period of convulsion as having been coextensive with the whole period

* There are indeed some faults which, as we shall presently see, affect the coal rocks and the superstrata in common: but these do not bear to the dislocations peculiar to the former, the proportion of 1 to 1000.

of the formation of these deposits, but restricted within much narrower limits, towards the very close of the epoch of these formations; as we find all the coal-measures, even the very newest beds of the formation, equally affected by the same dislocations.

Now the geological period thus limited cannot surely be magnified (except by the optics of a very convenient faith) into a series of ages of indefinite extent. I would then only request any person at all acquainted with the subject, or in the least competent to form an opinion, to compare these vast and general convulsions of our coal-fields, with all the utmost effects which the volcanic forces of the present period have been capable of producing for the 3000 years of which we possess historical information;—the most favourably coloured view of these actual convulsions may undoubtedly be found in Mr. Lyell's work, and to this I most willingly refer. I will only ask, whether these actual convulsions can be considered as bearing any sensible proportion to those of the carboniferous formations. If therefore we suppose that the disturbing forces are still acting with the same degree of energy as formerly, we must make large draughts on time, to enable the frequent repetition of the minor dislocations to produce aggregate effects equal to the greater: and thus we shall have the following proportionals, As the actual convulsions are to those of the coal-fields (which may be taken almost as = 0 : infinite), so is 3000 years (the actual historical period) to the single geological epoch of the very close of the coal formation. I can only add that he who can believe this geological epoch to have been many million times 3000 years, must possess a somewhat larger imagination than I can lay any claim to.

[To be continued.]

LXII. *Analysis of a peculiar Submuriate of Iron, and of some other Subsalts.* By R. Phillips, *F.R.S. &c.*

HAVING occasion for a solution of peroxide of iron, that should contain as little excess of acid as possible, I mixed such quantities of muriatic acid and moist precipitated peroxide, as would form a compound of an atom of each. I preferred the muriatic acid to all others, on account of the facility with which it dissolves peroxide of iron in every state of aggregation. I was surprised to find that the acid took up much more of the oxide than I had anticipated; and after repeated additions of it, I procured a very deep red-coloured

coloured solution, which had but little of the well known chalybeate taste, and its specific gravity was 1·017; this solution is not decomposed either by the admixture of water, or the application of heat, unless it be evaporated to dryness; the alkalies readily decompose it, but the ferrocyanate of potash, instead of a deep blue, gives a dark brownish green precipitate; when more oxide is added to the acid than it is capable of dissolving, the excess, or a portion of it, combines with the submuriate already formed, and the acid and oxide are totally precipitated, forming another, but an insoluble submuriate of iron.

One of the most curious properties of the soluble submuriate, and in which it differs from all other binary salts with which I am acquainted, is its decomposition by an addition of its acid. To a quantity of the solution which contained nearly 7 grains of peroxide, I put 25 drops of muriatic acid; it occasioned immediate precipitation, and 3 grains of submuriate were thrown down: when however the solution is heated with excess of muriatic acid, no precipitation occurs.

To determine the composition of the solution of specific gravity 1·017, a thousand grains of it were boiled with a solution of potash; the precipitated peroxide of iron, after washing and drying, weighed 15·5 grains; the solution from which the oxide had been separated, was saturated with nitric acid, and treated with nitrate of silver, by which 6 grains of chloride were obtained: these experiments were repeated, with very slight variations in the results.

As 146 grains of chloride of silver are equivalent to 37 grains of muriatic acid, 6 denote 1·5 grain as the quantity of acid combined with 15·5 grains of peroxide of iron, consequently 37 (one atom) of muriatic acid combine with 382 of peroxide of iron, which divided by 40, the atom of peroxide, shows that one atom of acid is united with 9·5 atoms of peroxide. If however the peroxide had amounted to 16·3 instead of 15·5 grains, then the atomic constitution of the salt would have been 1 atom of acid + 10 atoms base; and this I am induced to suppose is its real composition; but it would require an extremely nice adjustment of the constituents to obtain it, for, as already stated, any excess of the peroxide of iron decomposes the soluble submuriate.

The submuriate which I have now described, is remarkable on several accounts; as far as I know, it is the only subsalt which is largely soluble in water, except the subacetate of lead; nor do I remember any one which contains so small an atomic proportion of acid; there is not, perhaps, any other binary

binary compound which is decomposed by the addition of either the acid or base; and the last-mentioned circumstances show, that there are two submuriates of peroxide of iron, in addition to that now described, and differing from it in being insoluble in water.

Another subsalt which I have examined, is the submuriate of antimony; this compound has been long known and employed by the name of Powder of Algaroth, yet I have not found an analysis of it in any chemical author.

To ascertain its composition, 100 grains were boiled in a solution of 200 grains of crystallized carbonate of soda, by the action of which, the muriatic acid is perfectly separated; the protoxide of antimony obtained, weighed 92 grains; the solution was slightly supersaturated with nitric acid, and by this, 0·5 of a grain of oxide, which had been dissolved by the carbonate of soda, was precipitated. To the solution, nitrate of silver was added, which gave 31·6 grains of chloride, equivalent to 8 of muriatic acid. On repeating this experiment, 92·4 of oxide and 30 of chloride were procured, giving 7·6 as the proportion of acid.

The mean of these experiments shows that submuriate of antimony is composed of

Protoxide of antimony	92·45
Muriatic acid...........................	7·80
	100·25

Estimating the atomic weight of protoxide of antimony at 52, and that of muriatic acid at 37, it appears that submuriate of antimony is constituted of

Nine atoms of protoxide of antimony	$52 \times 9 =$	468 or	92·67
One atom of muriatic acid............	$=$	37	7·33
		505	100·00

Subnitrate of bismuth, as occupying a place in the London Pharmacopœia, is a salt of some importance. I have met with only two notices of its composition; one, confessedly theoretical, is in Mr. Brande's Table of Proportionals, and the other is given by Mr. Reid, in his Elements of Chemistry, in the form of a diagram; these authors both represent the salt in question as a dinitrate: this I shall presently show is not the case; indeed, according to Dr. Thomson, the dinitrate is a crystalline salt.

In the Pharmacopœia this subnitrate is directed to be prepared, by dissolving an ounce or 480 grains of bismuth in a fluid ounce and half, or about 680 grains of strong nitric acid diluted with half its bulk of water. This proportion of acid is

is considerably too large, and the excess prevents the precipitation of a large portion of the subnitrate by water: thus when 480 grains of the metal were dissolved as above directed, water threw down only 257 grains of subnitrate; a solution of common salt precipitated afterwards 307 grains of submuriate, and ammonia then gave 27 grains more of the latter subsalt.

It will be observed, that the subnitrate precipitated by water is to the submuriate thrown down by common salt, only about as 100 to 120; and that this deficiency of subnitrate is occasioned by the great excess of acid, is found by using crystallized nitrate of bismuth: in this case the subnitrate obtained by water was to that of the submuriate procured by common salt, nearly as 100 to 33, instead of 120. To analyse subnitrate of bismuth, 200 grains were boiled in a solution of soda; taking the mean of several experiments, 81·92 per cent of oxide of bismuth were left: to determine the proportion of nitric acid, a similar quantity was heated in water with excess of lime; after filtering the solution, carbonic acid gas was passed into it, until the carbonate of lime formed began to redissolve; the excess of carbonic acid was expelled by ebullition, and the nitrate of lime being decomposed by carbonate of soda, 17 per cent. of carbonate of lime were obtained as the mean of two experiments, which quantity is equivalent to 18·36 of nitric acid.

According to these experiments, this subsalt consists of

Oxide of bismuth............	81·92
Nitric acid....................	18·36
	100·28

The atom of oxide of bismuth being 80, and that of nitric acid 54, it appears that subnitrate of bismuth is constituted of

Three atoms of oxide 80×3	=	240 or	81·64
One atom of acid..............	=	54	18·36
		294	100·00

I have also subjected the submuriate, formerly called magistery of bismuth, to examination; it was prepared by adding a solution of common salt to one of nitrate of bismuth. In analysing this salt, I adopted a similar process to that employed with the submuriate of antimony; viz. ebullition in a solution of soda, saturation with nitric acid, and precipitation by nitrate of silver.

One hundred grains treated in this manner gave 87 grains of oxide of bismuth, and 54 of chloride of silver, equivalent to

to 13·6 of muriatic acid, showing the composition of the salt to be,

Oxide of bismuth......	87·0
Muriatic acid..........	13·6
	100·6

The atom of oxide of bismuth being 80, and that of muriatic acid 37, it appears that this salt is constituted of

Three atoms of oxide of bismuth...	80×3	=	240 or	86·6
One atom of muriatic acid...........		=	37	13·4
			277	100·0

Dr. Thomson found that the carbonate of bismuth is a triscarbonate, similar in constitution to the subnitrate and submuriate as above stated.

It is well known that the oxide of bismuth is of a yellow colour; and this I have always found to be the case when it is procured by decomposing the subnitrate by an alkali; but when the submuriate is employed, the colour of the oxide frequently differs exceedingly; sometimes it is yellow like that from the subnitrate, frequently grayish black, and I once obtained it of a deep bluish black colour.

The cause of these variations of colour I have not been able to discover, nor is there any circumstance occurring before the actual decomposition, which gives any indication of what colour the oxide will be. I have used portions of the same solution of nitrate of bismuth in preparing the subnitrate and submuriate; and equal weights of the salts obtained, one by water and the other by common salt, were boiled in similar quantities of the same solution of soda; the subnitrate gave the usual yellow oxide, and the oxide from the submuriate was yellowish at first, but it soon became grayish, and finished by being nearly black.

In other cases when using bismuth taken from one mass, and dissolving and precipitating as before, the submuriate has, like the subnitrate, yielded a yellow oxide. The only circumstances which appear to be constant are, that the subnitrate always gives a yellow oxide; and any one portion of submuriate always yields a dark coloured oxide, though, as already observed, the submuriate obtained at different times, even when using portions of the same mass of bismuth, gives very different results.

I could not detect any impurity in the bismuth; indeed had this or the accidental action of sulphuretted hydrogen blackened the oxide of the submuriate, it must have produced a similar effect upon the oxide from the subnitrate, which however

ever did not occur, though both were decomposed together on the sand heat.

If the black precipitate be dissolved in nitric acid, nitrate of silver does not indicate the presence of any muriatic acid; indeed whether the oxide was yellow or black, there was no material difference in the quantity of chloride of silver yielded by the decomposition of the submuriates: nor was there any greater variation, than usually occurs in the results of experiments, between the proportions of yellow or black oxide, procured from equal portions of the submuriate.

When the black oxide is dissolved in muriatic acid, water occasions a precipitation of submuriate of the usual whiteness; a portion of the black oxide heated to redness on a piece of platina lost neither weight nor colour, but by melting it became yellow; and the last-mentioned circumstance is the only one which affords even a guess as to the cause of the variations of colour, and that is the state of aggregation of the particles: but why it should not occasionally occur with the subnitrate does not easily admit of explanation; and it is, perhaps, still more difficult to account for the uncertainty which attends the results of different portions of submuriate, obtained from one mass of bismuth.

LXIII. *Notes on the New Red Sandstone of the County of Durham, below the Magnesian Limestone.* By Wm. Hutton, Esq. *F.G.S.*

[Concluded from p. 354.]

AT the south point of Cullercoats Haven, the great, or ninety-fathom dyke, as it is called, again brings down the magnesian limestone and the yellow sand. The dyke may be seen in the cliff, near the south point of the haven, where a coal sandstone and a bed of shale form its high or southern cheek, and the yellow sand (here a soft sandstone) the northern. The dyke hades, or underlies about 38° to the north, and its direction is N. 87° W. Its course towards the sea may be traced without difficulty, at low water, for a considerable distance eastward, the well-known sandstone rock, called the "Bear's Back," forming its southern side, and the yellow sand having many thin beds of magnesian limestone alternating with it, the northern. These alternating beds of limestone and sand show marks of considerable mechanical force, being bent and contorted near the edge of the dyke. Within the bay a bed of shale is exposed to view, which here forms the southern cheek of the dyke, in consequence of the action of the sea

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having removed the whole of the yellow sand, except at the south-eastern point, where the curved beds of limestone may be again seen alternating with the sand, as well as in the cliff below the Fisherman's Beacon*.

From the appearances at this point it cannot be doubted that the dyke has thrown down the magnesian limestone, as Professor Sedgwick observes; and it also follows, as a matter of course, that the limestone at Whitley Quarry, upon the course of the dyke, is similarly affected. A close examination of the quarry last autumn convinced me that such was the fact; the operations of the quarrymen had removed, in one spot, the whole of the limestone, and laid bare, for a considerable distance, the southern cheek of the dyke, which was here, as in the Haven at Cullercoats, a bed of shale, having a hade or dip, at a considerable angle, towards the north. On the southern side of the quarry, in several places where the stone has been worked near the line of the dyke, marks of mechanical action are visible, particularly near the rail-way, on its eastern side.

The general opinion is, that this patch of limestone overlies both edges of the dyke, and that it has been deposited not only after the slip took place, but after the removal of the whole of the high side, which would necessarily be left, by the sinking down of the strata on the north. This is an opinion from which I confess I differ with reluctance; nevertheless, as the limestone at Cullercoats is manifestly thrown down along with the yellow sand, and contorted by mechanical action, we are compelled to come to the conclusion, that the ninety-fathom dyke was formed after the deposition and consolidation of the magnesian limestone; and this would necessarily be our conclusion if there were no marks in the quarry at Whitley to point it out, as we cannot suppose the limestones in the two situations to be of different ages, or, closely connected as they are, to be operated upon by different causes.

The idea of the limestone overlying the dyke, may possibly have arisen from its being considered as a perpendicular fissure, which it certainly is not, either in the quarry at Whitley, at Cullercoats, or at Gosforth, where it has lately been so completely examined in Mr. Brandling's new colliery.

We have thus traced the edge of this formation through the whole of the county of Durham, and to Cullercoats, in Northumberland, its most northern limit; and, in the whole line, we have seen the yellow sand and red sandstone accom-

* In Professor Sedgwick's Section (Geol. Trans. 2nd Series, vol. iii. pl. v. fig. 2.) the yellow sand, thrown down by the dyke, is coloured as magnesian limestone, which is a mistake, the limestone existing only in thin beds, subordinate to the sand, which is here of great thickness.

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panying the magnesian limestone: the series of specimens now before the Society, from the different localities, will show most of the characters of the two beds. At the same time it must be admitted, that hand specimens can give but a vague idea of a formation of such extent and variety as this is. In many situations on the line it might be taken by any one who had not examined it thoroughly, to be a sandstone of the coal measures; but a more extensive survey, with an attention to all the circumstances under which it occurs, could not fail of satisfactorily pointing out its true relations to the adjoining strata.

The most convincing proof of its total independence of, and want of conformity to, the coal measures, is the difference of depth at which the same seam of coal is found along the line of its outcrop. If we take, for instance, the low main coal of the Tyne, which is the Hutton seam in the collieries on the Wear, we shall find its depth below the red sandstone, as follows:—

	Fathoms.
At the foot of the cliff below Tynemouth Castle it will be about	75*
At Laygate Quarry about	140
At Clacksheugh, at least	230
At Houghton-le-Spring (Lord Durham's new pit)	132
At Moorsley (Mr. Russell's pit)	95

To the south of Moorsley, the seams of coal unfortunately again change their names, and it would therefore be impossible, without further investigation, to trace the continuity of each individual seam; but it may be stated, that the sandstone crosses the coal strata, at many various depths, above a coal in that district, called the Five-quarter Seam, and in its range southward very nearly comes in contact with the grit and shale beds below the whole of the coal series.

	Fathoms.
At Quarrington Pit from the red sandstone to the Five-quarter Seam, is	37
At Eldon Pit from the red sandstone to the Five-quarter Seam, is	9
And at Cowndon Pit from the red sandstone to the Five-quarter Seam, is	57

The source of the brine springs, which are found in several situations in this neighbourhood, has long been a matter of

* This estimate is made upon the idea of the seam of coal seen immediately beneath the Two-gun Battery, at the south end of Cullercoats sands, being the High Main; but as there is reason to believe, from the very best authority (Mr. Buddle), that it is either the Bensham, or Yard Coal, it is probable the Low Main Coal is much nearer the red sandstone than above stated.

interesting

interesting speculation. As it has now been ascertained that we have below the magnesian limestone a formation of new red sandstone, may we not be allowed to conjecture that a bed of rock salt is existing in it somewhere, as this stratum is well known to be the great depository of that substance all over the world? This idea is rendered more probable by the situation of the places where salt springs occur, none of which are at a great distance from the outcrop of this stratum. Butterby, near Croxdale, is about two miles from it; Lumley is rather less than two miles; Old Walker Colliery is furthest from the line, being about three miles and a half; and Jarrow is the nearest, being little more than a mile and a half*.

The great scarcity of organic remains in the lower beds of the magnesian limestone is rather singular. In the foregoing notice, four different localities have been mentioned, where fish have occurred, all in the slaty beds of the limestone, a few feet above the yellow sand. Organic remains are at all times of great use to the geologist and observer of nature; they are, as it were, nature's own medals of the wonderful changes that have taken place upon the surface of the globe before it was brought to its present state; and the great importance of these fish is, the light they may throw upon the nature of the changes that have taken place at the time they were buried. That the catastrophe was sudden, the forms in which they occur, and their perfect preservation, sufficiently testify; indeed, it is a generally received opinion, that where the remains of soft-bodied animals occur, with their outward form perfectly preserved, and associated in families, they have been suddenly overwhelmed, and entangled in the substance now forming their stony matrix. It is a singular circumstance attending these fish, and one in which they agree with those that have been found in different situations upon the continent, that many of them are contorted; not that sort of twisting which might be produced by any movement in the mass, and subsequent to the time they were enveloped, but the graceful contortions of the living animal in a state of pain, as if struggling against its fate.

Postscript to Mr. Hutton's Notes on the New Red Sandstone, of the County of Durham, below the Magnesian Limestone†.

During the progress of the foregoing notes through the press, we were induced to examine the effects upon the

* Besides the places above enumerated, brine springs are common in the collieries of Hebburn, Wallsend, and Percy Main.

† Read May 18th, 1830.

strata

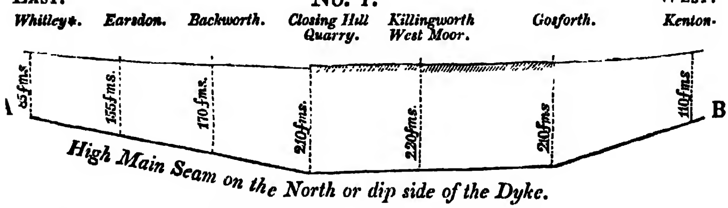
strata caused by the ninety-fathom dyke, at the point of its greatest depression, certain appearances having led us to suppose that the lower new red sandstone existed somewhere near Killingworth; and we found it accordingly in one spot, called the Clowsden or Closing Hill Quarry, situate about 950 yards to the south of Killingworth House, and immediately adjoining the Killingworth Railway. It is of inconsiderable extent, and forms a small hill, which slopes gently on every side, except where it has been broken in upon for quarrying the stone. There are two quarries: that on the northern slope of the hill has been extensively worked; it is now full of water, but is said to be sixty feet deep. The southern quarry was drained by means of a drift from the bottom of the hill; this was driven northward entirely in sand, until the face of the rock was suddenly and abruptly come upon, which was no doubt the northern cheek of the dyke. I am informed by my friend Mr. Nicholas Wood, that a seam of coal twenty inches thick, with a shale bed above it, appeared in the north quarry; this coal stratum is higher than any bed we have been hitherto acquainted with in this coal field. The highest known is in Hebburn, Jarrow, and South Shields collieries, from their pits being sunk at the point of the greatest depression of the strata, or at the bottom of the coal basin, as it is termed; it is 114 fathoms above the High Main, whilst this is 190 fathoms at least.

The red sandstone exhibits here its usual characters, but the ruddle is in greater abundance than common, particularly in the lower part of the bed, where it exists in large masses, all the farmers in the neighbourhood supplying themselves from this quarry with Keel (as it is termed, and the spot the Keel Quarry), for marking their sheep. Its dip is 15° south, whilst the dip of the coal measures is twice this amount, or about 30°. It is satisfactory to know, that this has been proved by the working of the High Main Seam below, because it would appear that the dip of the coal seam and shale bed which were found in the quarry did not differ much from that of the red sandstone: nevertheless I am perfectly borne out by the personal observation and practical experience of Mr. Wood, in considering the red sandstone here "in general position clearly unconformable with the coal measures."

The relative position of this patch of red sandstone will be best understood by a reference to the annexed diagrams. In the sketch No. 1. the line A B represents the course of the High Main Coal Seam, on the north, or low side of the dyke, having the depth marked at which the coal has either been worked, or proved to exist, at seven different points; and shows

shows the remarkably undulating line each bed takes, by the unequal depression of portions of the strata. I have taken the High Main Coal as the representative of the whole series, from its being best known; but the continuous line exhibited, although sufficient for our present purpose, is incorrect, as many slips or dykes occur, throwing it down in portions unequally.

EAST. No. 1. WEST.

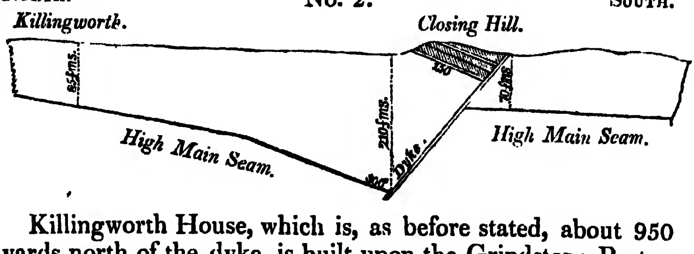


The amount of "throw" caused by the dyke at the points named in the diagram No. 1, will be nearly as below:—

At Whitley.	Earsdon.	Backworth.	Under Closing Hill Quarry.	Killingworth West Moor.	Gosforth.	Kenton.
100 faths.	150 faths.	160 faths.	140 faths.	175 faths.	170 faths.	120 faths.

The diagram No. 2. represents an ideal section of the strata from Killingworth village southward, through the Closing Hill Quarry. It is a remarkable circumstance that, although the slip, or "throw," is here so enormous, yet, that the derangement, arising from the increase of inclination of the strata, extends but a very short distance from the dyke.

NORTH. No. 2. SOUTH.



Killingworth House, which is, as before stated, about 950 yards north of the dyke, is built upon the Grindstone Post, a well-known sandstone bed here at the surface; but, if we wished to find that bed at the dyke, we should have to sink 120 fathoms before we reached it.

The occurrence of the red sandstone, in the situation described, affords evidence of great value in estimating the cor-

* The depth at which the High Main Coal is worked in Whitley Colliery, on the north of the dyke, is, by mistake, stated to be fifty fathoms in the 4th volume of the Geological Transactions, page 25.

rectness

rectness of the views taken of this stratum in the foregoing notice. That this patch of sandstone, which is now upwards of six miles from the nearest point of the same rock, once formed part of a continuous stratum, we cannot doubt, nor that the intervening portion has been removed by the operation of water, that mighty agent which has been employed universally in modifying the surface of the globe. It is difficult to obtain an idea of the extent of force necessary, but it is, nevertheless, as probable, that such a removal of this bed may have taken place, as that the strata on the high side of the dyke have been removed, which, when the slip took place, must have presented at this point, a face of rock, upwards of one thousand feet high.

LXIV. *Additions to the Theory of Eclipses, and the Methods of calculating their Results.* By *Professor* Bessel.

[Concluded from page 347.]

LET us now suppose that ϕ', μ denote the latitude and longitude of the zenith; D, A the longitude and latitude of the star; let (μ) and (φ') express the right ascension and declination of the zenith, and ϵ the obliquity of the ecliptic. We then obtain these equations:

$$\sin \phi' = \sin (\varphi') \cos \epsilon - \cos (\phi') \sin (\mu) \sin \epsilon$$

$$\cos \phi' \sin \mu = \sin (\phi') \sin \epsilon + \cos (\varphi') \sin (\mu) \cos \epsilon$$

$\cos \phi' \cos \mu = \cos (\varphi') \cos (\mu)$, from which we derive the following expressions for u and v by (φ') and (μ)

$$u = r \sin (\varphi') \sin \epsilon \cos A + r \cos \varphi' [\cos A \sin (\mu) \cos \epsilon - \sin A \cos (\mu)]$$

$$v = r \sin (\phi') . [\cos D . \cos \epsilon - \sin D \sin \epsilon \sin A]$$
$$- r \cos (\phi') [\sin (\mu) (\cos D \sin \epsilon + \sin D . \cos \epsilon \sin A) + \cos (\mu) \sin D \cos A]$$

The differential quotients of $r \cos (\varphi')$ and $r \sin (\phi')$ give therefore, if β retains the above signification,

$$\frac{d u}{d . e^2} = \tfrac{1}{2} \beta^2 . u - \beta . \sin \epsilon \cos A$$

$$\frac{d v}{d . e^2} = \tfrac{1}{2} \beta^2 . v - \beta (\cos D \cos \epsilon - \sin D . \sin \epsilon . \sin A)$$

We have therefore, calculating with longitudes and latitudes, and referring $N + \psi$ to the same, for the term dependent on Δe^2, this expression:

$$- \omega \sin \pi . \Delta e^2 \left\{ \tfrac{1}{2} \beta^2 [x \cos \psi + \frac{n}{s} (t - d - \tau) \sin \psi - k] + \right.$$

$\beta \sin \varepsilon \;\cos A \;\cos (N+\psi) - \beta \;(\cos D \cos \varepsilon -$
$\sin D \sin \varepsilon \sin A) \sin (N+\psi) \}$.

All the parts taken together, give the following complete development of formula (6).

$$(11) \ldots d = t - T + \frac{m\,s}{n}\,\frac{\cos(M-N-\psi)}{\cos\psi} - h\,.\,\frac{\cos(N+\psi)}{\sin\psi}\cos\delta\,\Delta\,\alpha$$
$$+ h\,\frac{\sin(N+\psi)}{\sin\psi}\,\Delta\,\delta + h\,.\,\frac{1}{\sin\psi}\,.\,\omega\sin\pi\,.\,\Delta\,k$$
$$- h\left\{\frac{x}{\text{tang}\,\psi} + \frac{n}{s}(t-d-\tau)\right\}\cos\pi\,.\,\Delta\,\pi$$
$$- h\left\{\tfrac{1}{2}\beta^2\left(\frac{x}{\text{tang}\,\psi} + \frac{n}{s}(t-d-\tau) - \frac{k}{\sin\psi}\right) - \frac{\beta V}{\sin\psi}\right\}\omega\sin\pi.\Delta e^2$$

in which V, if we calculate with right ascension and declination, $= \cos D \sin(N+\psi)$, and if we calculate with longitudes and latitudes

$= (\cos D\,.\cos\varepsilon - \sin D \sin\varepsilon \sin A) \sin (N+\psi) -$
$\sin\varepsilon \cos A\,.\cos(N+\psi)$.

It appears from this formula that there are combinations between the quantities $\Delta\,\alpha$, $\Delta\,\delta$, &c. in which they affect the result, or that several of them will appear united by the equality of their coefficients. This is expressed by this formula:

$$(12) \ldots d = t - T + \frac{m\,s}{n}\,\frac{\cos(M-N-\psi)}{\cos\psi} + h\,\varepsilon + \frac{h\,\zeta}{\text{tang}\,\psi} + \frac{h\,\eta}{\sin\psi} - h\,E\,\vartheta - h\,F\,.\,i$$

in which $\varepsilon = \sin N\,.\cos\delta\,.\,\Delta\,\alpha + \cos N\,.\,\Delta\,\delta$

$\zeta = -\cos N\,.\cos\delta\,.\,\Delta\,\alpha + \sin N\,\Delta\,\delta - x\,.\cos\pi\,.\,\Delta\,\pi$
$\eta = \omega\sin\pi\,.\,\Delta\,k$
$\vartheta = \cos\pi\,.\,\Delta\,\pi$
$i = \omega\sin\pi\,.\,\Delta\,.\,e^2$

and E and F represent the above-given coefficients of $h\,\vartheta$ and $h\,i$. This form is the most simple in which the influence of the corrections of the elements of calculation can be represented.

The unknown quantity ε cannot be determined by observations of an occultation of a star, except when d is known for at least one place of observation, for it entirely unites with the difference of meridians. The second term proportional to the reciprocal of the tangent ψ is the same which is usually added to the time of conjunction deduced from an observation as correction on account of the latitude (or declination); but it appears from the expression for its coefficient ζ, that this term involves $\Delta\alpha$, $\Delta\delta$, and $\Delta\pi$. The dependence of this term on $\Delta\,\alpha$ might appear contradictory to the first method above explained

[1],

[1], as the time of conjunction was there determined without being affected by an error in longitude (or right ascension), whereas such an error here affects the difference of meridians, and consequently likewise the time of conjunction. But it is to be observed that this contradiction is only apparent, and may be considered as arising from the omission of that term of the correction which is to be added to the time of the conjunction calculated by that method, and which depends on the error (a) of the assumed difference of meridians as explained in [2]; if this correction is added and x eliminated by the comparison of the time of conjunction found with the one resulting from the tables and $\Delta\ \alpha$, the dependence on this quantity will likewise be perceived. Both methods only differ by being made to depend on different unknown quantities. The determination of all the 5 quantities $\varepsilon, \zeta \ldots$ by observations of an occultation of a star, is mathematically possible; but it is easily perceived that if these quantities are thus kept separate, small errors of observation will greatly affect their determination, unless the observations were made at the most proper spots of the globe, and not confined to the small part of its surface contained between the observatories of Europe. The advantageous separation of the two last quantities from the others, requires, for example, that at two of the places of observation the times of the phænomenon should be very different, which will be the case if at the one it takes place a little after the rising of the moon, and at the other a little before her setting; the last is separated from the rest only by the difference of the value of β for the different places of observation.

The difficulty of producing a concurrence of favourable circumstances induces the belief that the determination of the excentricity of the meridians of the earth for which observations of occultations of stars have been proposed (without, however, sufficiently developing the greatest possible advantages to be obtained by them), may always be founded on more successful methods. Besides this difficulty, the mountains projecting on the limb of the moon, and other probable deviations from the globular form, may and will spoil observations good in themselves; the immersion and emersion can rarely both be observed with accuracy; and lastly, the advantageous selection of the places of observation is much restricted by the presence of the sun above the horizon. I believe, therefore, that the calculation of the influence of all the five unknown quantities will only have an interest for the purpose of judging how far they may affect the results of the calculation, but not for their determination.

[11.] In most cases only ε and ζ will be determined by the observation; in particular cases, likewise η; but the others will be considered as evanescent. My experience proves that this object, a very limited one when compared with the complete determination of all unknown quantities, is generally so difficult to be attained that, in most cases, a good meridian observation of the moon is very acceptable in order to diminish the uncertainty which the occultation alone leaves behind. The comparison of it with the observations of the occultation is most easy when the right ascension and declination have been employed in the calculation. The quantities ε and ζ having been found by observations of an occultation, we have

$$(13) \ldots\ldots \begin{cases} \cos\delta\,\Delta\alpha = \varepsilon\sin N - \zeta\cos N \\ \Delta\delta = \varepsilon\cos N + \zeta\sin N \end{cases}$$

If these quantities denote the errors of right ascension and declination, and if it be required to find those of longitude and latitude, or *vice versâ*, the well-known formulæ by which these calculations may be effected are to be applied. The complete formulæ [11] and [12] will show in every case how far the errors of the tables determined on the supposition of η, ϑ, i being evanescent might be altered by these quantities. This connection might be determined generally from one of the two formulæ in particular cases, *ex. gr.* if $\Delta\alpha$ and $\Delta\delta$ have been determined by the combination of the observations of immersion and emersion made at one place; but it appears to be more convenient to calculate the coefficients for both phænomena, and to derive the result required from their numerical values.

[12.] I shall now generally consider the problem of eclipses, and suppose that both bodies have a parallax and a diameter. The determination of the most convenient form of the general equation [2] is then less apparent than in the particular case of an occultation of a fixed star; but even then formulæ may be found combining convenience of calculation with perfect correctness. Although the method of approximation, explained by Lagrange, is sufficient for practice, yet the importance of a theory which has been so often treated, will be an apology for resuming it again.

The expression $(a\,b' - a'\,b)\,(c'\sin\pi - c\sin\pi') - (a\,c' - a'\,c)\,(b'\sin\pi - b\sin\pi') + (b\,c' - b'\,c)\,(a'\sin\pi - a\sin\pi')$ is identically $= 0$: if we put, therefore,

$$c'\sin\pi - c\sin\pi = G\sin d$$
$$b'\sin\pi - b\sin\pi' = G\cos d\cos a$$
$$a'\sin\pi - a\sin\pi' = G\cos d\sin a$$

and

and substitute d and a for the arbitrary quantities u and v used in the transformation of the sum of three squares, we shall have $(a\,b' - a'\,b)^2 + (a\,c' - a'\,c)^2 + (b\,c' - b'\,c)^2 = [(a\,b' - a'\,b)\cos d + (a\,c' - a'c)\sin d\cos a - (b\,c' - b'\,c)\sin d\sin a]^2 + [(a\,c' - a'\,c)\sin a + (b\,c' - b'\,c)\cos a]^2$, and the expression which forms the first part of the equation (2) is thus reduced to the sum of two squares.

The angles d and a by which this is effected may be considered, the first as the declination (or latitude), the second as the right ascension (or longitude) of a point of the sphere of the heavens, which may be easily demonstrated to be the point in which the great circles passing through the true and apparent places, respectively, of the bodies, intersect each other. For in the expressions by which d and a have been determined, the last parts of the expressions [1] of a, b, c, a', b', c' vanish; so that we have

$$(14)\ldots\begin{cases} G\sin d = \sin\pi\sin D - \sin\pi'\sin\delta \\ G\cos d\,.\cos a = \sin\pi\cos D\cos A - \sin\pi'\cos\delta\cos\alpha \\ G\cos d\,.\sin a = \sin\pi\cos D\sin A - \sin\pi'\cos\delta\sin\alpha \end{cases}$$

In these equations is contained the condition that the three points concerned in it, viz. the two true places of the bodies and the point determined by d and a, are situated in a great circle; this condition may be reduced to the form in which it is usually represented, by eliminating G, $\sin\pi$, $\sin\pi'$, which is done by multiplying the three equations respectively by

$$-\frac{\sin(\alpha - A)}{\cos\delta},\ +\frac{\tan d}{\cos\delta}\sin A - \frac{\tan D}{\cos\delta}\sin a,\ -\frac{\tan d}{\cos\delta}\cos A$$
$$+\frac{\tan D}{\cos\delta}\cos a, \text{ and we shall have}$$

(15) ...... $o = \tan\delta\sin(A - a) - \tan D\sin(\alpha - a) + \tan d\sin(a - A)$ the usual form of the condition above mentioned. But as we have likewise

$$\begin{aligned} G\sin d &= \sin\pi\,.\,\Delta'\,.\sin D' - \sin\pi'\,.\,\Delta\,.\sin\delta' \\ G\cos d\,.\cos a &= \sin\pi\,.\,\Delta'.\cos D'\cos A' - \sin\pi'\,\Delta\cos\delta'.\cos\alpha' \\ G\cos d\,.\sin a &= \sin\pi\,.\,\Delta'.\cos D'\cos A' - \sin\pi'\,.\,\Delta\cos\delta'.\sin\alpha \end{aligned}$$

And as these equations have the same form as the preceding ones, the point determined by d and a is likewise situated in the great circle passing through the apparent places. Substituting for a, b, c, a', b', c' their expressions in [1] we obtain

$$a\,b' - a'\,b = \cos\delta\,.\cos D\sin(\alpha - A) - G\,.\,r\cos\varphi'\cos d\,.\sin(\mu - \alpha)$$

$$a\,c' - a'\,c = \cos d\,.\sin D\sin\alpha - \cos D\,.\sin\delta\sin A - G\,[r\cos\varphi'\sin d\sin\mu - r\sin\varphi'\cos d\,.\sin\alpha]$$

$b\,c'$ —

$$b\,c' - b'\,c = \cos\delta\sin D\cos\alpha - \cos D\sin\delta\cos A - G\,[r\cos\varphi'\sin d\,.\cos\mu - r\sin\varphi'\cos d\,.\cos a]$$

and consequently,

$$(a b' - a' b)\cos d + (a c' - a' c)\sin d\,.\cos a - (b' c' - b\,c)\sin d\sin a$$
$$= -\sin\delta\cos D\sin d\sin(A - a) + \cos\delta\sin D\sin d\,\times$$
$$\sin(\alpha - a) + \cos\delta\cos D\cos d\sin(\alpha - A) - G\,.\,r\cos\varphi'\sin(\mu - a)$$

and adding the product of equation [15] by $\cos\delta\cos D\sin d$

$$= \frac{\cos D}{\cos d}\cos\delta\sin(\alpha - A) - G\,.\,r\cos\varphi'\sin(\mu - \alpha)$$

We likewise obtain

$$(a\,c' - a'\,c)\sin a + (b\,c' - b'\,c)\cos a = \cos\delta\sin D\cos(\alpha - a)$$
$$-\sin\delta\cos D\cos(A - \alpha) + G\,[r\sin\varphi'\cos d - r\cos\varphi'\sin d\,\times$$
$$\cos(\mu - \alpha)].$$

The second part of equation [2], viz. $(a'\sin\varrho \pm a\sin R)^2 + (b'\sin\varrho \pm b\sin R)^2 + (c'\sin\varrho \pm c\sin R)^2$ is more convenient for calculation, if represented in its irrational form. It is the square of $\Delta\,.\,\Delta'\sin\Sigma = \sin\varrho\,\surd\,(\Delta'^2 - \sin R^2) \pm \sin R\,\surd\,(\Delta^2 - \sin\varrho^2)$ where

$$\Delta^2 = a^2 + b^2 + c^2 = 1 - 2r\sin\pi\,.\cos\gamma + r^2\sin\pi^2$$
$$\Delta'^2 = a'^2 + b'^2 + c'^2 = 1 - 2r\sin\pi'\,.\cos\gamma' + r^2\sin\pi'^2$$

$\cos\gamma$ and $\cos\gamma'$ being written for

$\sin\varphi'\sin\delta + \cos\varphi'\cos\delta\cos(\mu - \alpha)$ and

$\sin\varphi'\sin D + \cos\varphi'\cos D\cos(\mu - A)$. If we denote, therefore,

$\surd[\cos\varrho^2 - 2r\sin\pi\cos\gamma + r^2\sin\pi^2]$ by λ

$\surd[\cos R^2 - 2r\sin\pi'\cos\gamma' + r^2\sin\pi'^2]$ by λ'

the required part is $(\lambda'\sin\varrho \pm \lambda\sin R)^2$.

[13.] The equation [2] becomes by substituting these transformations of its several parts:

$$(16)\ \ldots \left(\frac{\lambda'\sin \pm \lambda\sin R}{G}\right)^2$$
$$= \left\{\frac{\cos D}{\cos d}\,.\,\frac{\cos\delta\sin(d - A)}{G} - r\cos\varphi'\sin(\mu - a)\right\}^2$$
$$+ \left\{\frac{\sin\delta\cos D\cos(A - \alpha) - \cos\delta\sin D\cos(\alpha - a)}{G} - r\,(\sin\varphi'\cos d - \cos\varphi'\sin d\cos(\mu - \alpha)\right\}^2$$

It has consequently induced the form $k^2 = (P - u)^2 + (Q - v)^2$, *the same* which takes place for the case of occultations of fixed stars. The difference between the general equation and the particular case consists in this, that in the former there is, instead of the constant k^2, a variable one dependent on the place of

of observation and the angles γ and γ' (the zenith distances of the bodies); that P and Q likewise involve d and a, and that u and v contain these angles instead of D and A. It is therefore not necessary to give particular methods for the calculation of eclipses, be they either eclipses of the sun or transits of the inferior planets over the disc of the sun, as all these phænomena may be treated after the method which I have developed for the occultations of stars.

From the formulæ [14] results

$$G^2 = \sin \pi^2 - 2 \sin \pi . \sin \pi' . \cos \sigma + \sin \pi'^2$$

$$\text{tang}\,(A - a) = \frac{\sin \pi' \cos \delta \sin (\alpha - A)}{\sin \pi . \cos D - \sin \pi' \cos \delta \cos (\alpha - A)}$$

where σ stands for the geocentric distance of the two bodies. For a solar eclipse we may put

$$G = \sin \pi - \sin \pi'$$

$$a = A - \frac{\sin \pi'}{\sin \pi} (\alpha - A)$$

$$d = D - \frac{\sin \pi'}{\sin \pi} (\delta - D)$$ without causing in the calculation any perceptible deviation from the truth. The quantities whose introduction has so much contracted the formulæ will then be found almost without calculation, and the calculation of solar eclipses will in point of ease present only insignificant differences from those of occultations of stars. We have here another confirmation of the remark which one has so often occasion to make,—that the rigorous mathematical solution of astronomical problems ceases to require more difficult calculations than the approximately correct ones, as soon as one has succeeded in representing the former in its true shape.

F. W. Bessel.

LXV. *Continuation of the Table of Atomic Weights, and Notice of a new Scale of Equivalents.* By Mr. John Prideaux, *Member of the Plymouth Institution.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen, Plymouth, Aug. 8th, 1830.

I BEG leave now to send you the Table of acids and bases, and a description of the scale, which has already intruded on so many of your pages.

Table

TABLE *of Salifiable Bases.*

	Thomson.	Berzelius.	Mean.
Alumina..........	$\dot{Al}$ 2·25	$\dddot{Al}^2$ 642·334	2·2 $\dot{Al}$ (*a*)
Ox. antimony	$\dot{An}$ 6·5	$\dddot{An}^2$ 1912·904	6·5 $\bar{An}$ (*a*)
Baryta	$\dot{Ba}$ 9·75	956·88	9·66 *
Ox. bismuth	$\dot{Bs}$ 10·	$\dddot{Bs}^2$ 2690·752	10· $\dot{Bs}$ (*c*)
—— cadmium......	$\dot{Cd}$ 8·	796·767	8·
—— cerium........	$\dot{Ce}$ 7·25	674·718	7·
Sesquiox. cerium..	$\dddot{Ce}^2$ 15·5	1449·436	15·
Ox. chrome......	5·	$\dddot{Cr}^2$ 1003·638	10·02 $\dddot{Cr}^2$ (*b*)
—— cobalt	$\dot{Co}$ 4·25	468·991	4·4
Perox. cobalt.....	$\dddot{Co}^2$ 9·5	1037·982	9·8
Subox. copper....	$\dot{Cu}^2$ 9·	891·39	8·96
Ox. copper	$\dot{Cu}$ 5·	495·695	4·98
Glucina	$\dot{Gl}$ 3·25	$\dddot{Gl}^2$ 962·958	3·22 $\dot{Gl}$ (*a*)
Ox. gold.........	$\dot{G}$ 26·	$\dot{G}^2$ 2586·026	26 $\dot{G}$
Perox. gold......	$\dddot{G}$ 28·	$\dddot{G}^2$ 2786·026	28 $\dddot{G}$
Ox. iridium		Note (*c*)	13·42 $\dot{Ir}$ (*c*)
Sesquiox. ditto			27·84 $\dddot{Ir}^2$
Binox. ditto......			14·42 $\ddot{Ir}$
Trinox. ditto			15·42 $\dddot{Ir}$
Ox. iron	$\dot{F}$ 4·5	439·213	4·45
Sesquiox. ditto....	$\dddot{F}^2$ 10·	978·426	9·9
Ox. lead	$\dot{L}$ 14·	1394·498	14·
Lime	$\dot{Ca}$ 3·5	356·019	3·53
Lithia	$\dot{Li}$ 2·25	227·757	2·26
Magnesia.........	$\dot{Mg}$ 2·5	258·353	2·54
Ox. manganese....	$\dot{Mn}$ 4·5	455·787	4·53
Sesquiox. ditto ...	$\dddot{Mn}^2$ 10	1011·575	10·06

* Where the constituent sign corresponds in all three columns, it is stated only in the first, to save needless repetition of this troublesome work to the printer.

Binox.

TABLE (concluded).

	Thomson.	Berzelius.	Mean.
Binox. manganese	$\ddot{Mn}$ 5·5	555 785	5·53
Ox. molybdenum..	$\dot{Mo}$ 7·	....	7·
Subox. mercury ...	$\dot{M}$ 26·	$\dot{M}^2$ 2631·645	26·1 $\dot{M}^2$ (a)
Ox. ditto........	$\ddot{M}$ 27·	$\dot{M}$ 1365·822	13·55 $\dot{M}$
Ox. nickel........	$\dot{N}$ 4·25	469·675	4·4
Sesquiox. ditto	$\dddot{N}^2$ 9·5	....	9·8
Ox. osmium......			13·53 $\dot{Os}$ (c)
Binox. ditto......			14·53 $\ddot{Os}$
Quadrox. ditto....			16 53 $\ddddot{Os}$
Ox. palladium		Note (c)	7·7 $\dot{Pa}$
Binox. ditto			8·7 $\ddot{Pa}$
Ox. platinum			13·42 $\dot{Pt}$
Binox. ditto......			14·42 $\ddot{Pt}$
Potash	$\dot{Po}$ 6·	589·916	5 95
Sesq. ox. rhodium.	....	(c)	16·12 $\dddot{R}^2$
Ox. silver........	$\dot{Ag}$ 14·75	1451·607	14·63
Soda............	$\dot{So}$ 4·	390·897	3·96
Strontia	$\dot{Sr}$ 6·5	647·285	6·5
Ox. tellurium	$\dot{Tl}$ 5·	$\ddot{Tl}$ 1006·452	5· $\dot{Tl}$
Thorina	....	$\dot{Th}$ 844·9	8·45
Ox. tin..........	$\dot{T}$ 9·25	835·294	8·3
Binox. ditto	$\ddot{T}$ 9 25	935·294	9·3
Ox. titanium.....	$\dot{Ti}$ 5·	....	4·95
—— uranium	$\dot{U}$ 27	2811·36	27·5
Binox. ditto......	$\ddot{U}$ 28	$\dddot{U}^2$ 5722·72	56· $\dddot{U}^2$
Yttria	$\dot{Y}$ 5·25	501·840	5·14
Ox. zinc	$\dot{Z}$ 5·25	503·226	5·2
Zirconia	$\dot{Zr}$ 6·	$\dddot{Zr}^2$ 1140·476	5·6 $\dot{Zr}$ (a)

(*a.*) Alumina, glucina, zirconia; oxides of antimony, bismuth and mercury: see the notes on the respective metals appended to the Table of undecomposed bodies. (See *supra*, p. 163, *et seq.*)

(*b.*) Oxide of chrome.—Thomson, correcting his number for this oxide in the First Principles, makes it (Ann. Phil. second series, vol. vi.) chrome 1 atom, oxygen 1½ atom: a formula, of which even its high authority does not demonstrate an advantage commensurate with its verbal inaccuracy. In signification it corresponds with that of Berzelius.

(*c.*) Oxides of iridium, osmium, palladium, platinum and rhodium, are taken from the paper of Berzelius quoted in the note on these metals appended as above referred to; but with adjustment to the mean weight of chlorine.

(*d.*) Peroxide of uranium.—The constitution assigned by Berzelius to this oxide seems to me rather confirmed than invalidated by Thomson's experiments.

TABLE *of Acids.*

	Thomson.	Berzelius.	Mean.
Acetic..........	6 25	....	6·27 (*a*)
Antimonic.......	$\ddot{An}$ 7·5	$\dddot{An}^2$ 2112·9	7·5 $\ddot{An}$ (*b*)
Arsenious........	$\ddot{As}$ 6·75	1440·	6·73 $\ddot{As}$ (*c*)
Arsenic..........	$\dddot{As}$ 7·75	1640 08	7·73 $\dddot{As}$ (*c*)
Benzoic..........	15·	1509·55	15·1 (*d*)
Boracic..........	$\ddot{B}$ 3·	2$\dddot{B}$ 871·966	2·95 $\ddot{B}$ (*e*)
——— crystallized	....	....	5·2 $\ddot{B}$ *Aq*²
Carbonic........	$\ddot{C}$ 2·75	276·437	2·76
Chloric..........	$\overset{\cdot\cdot\cdot\cdot\cdot}{Cl}$ 9·5	942·65	9·46
Perchloric......	$\overset{\cdot\cdot\cdot\cdot\cdot\cdot\cdot}{Cl}$ 11·5	$\overset{\vdots\vdots\vdots}{Cl}$ 1042·65	11·46 $\overset{\cdot\cdot\cdot\cdot\cdot\cdot\cdot}{Cl}$ (*f*)
Chloriodic.......	Cl^2I 24·5	....	24·72 (*g*)
Chromic.........	$\dddot{Cr}$ 6·5	651·819	6·51
Citric...........	7·25	727·85	7·27 (*h*)
—— crystallized..	$\bar{C}$ *Aq*³ 9·5	$\bar{C}$ *Aq* [illegible]	{ $\bar{C}$ *Aq* $\frac{3}{2}$ 8·96
Columbic........	$\dot{Cb}$ 19·	$\ddot{Cb}^2$ 2607·43	19 $\dot{Cb}$ (*i*)
Ferroprussic.....	....	....	7·33 (*k*)

TABLE

TABLE (concluded).

	Thomson.	Berzelius.	Mean.
Fluoboric	4·25	....	4·25 $Fl\dot{B}$?
Fluoric	$\dot{F}n$ 1·25	FlH 2·43	$\begin{cases} 1·3\ \dot{F}n \\ 2·43\ FlH \end{cases}$
Formic	4·625	463·93	4·64 (*a*)
Gallic	7·75	791·78	7·85
Hydriodic	HI 15·625	....	16· (*g*)
Hydrobromic	....	....	10· (*g*)
Hydrocyanic	C^2NH 3·375	342·39	3·4
Iodic	$\dot{I}$ 20·5	2078·29	20·8
Lactic	5·75	....	5·78 (*a*)
Molybdic	$\dddot{M}o$ 9·	898·525	9·
Molybdous	$\ddot{M}o$ 8·	....	8·
Muriatic	ClH 4·625	455 13	4·58
Nitric	$\dot{N}$ 6·75	677·036	6 76
Nitrous	$\dot{N}$ 5·75	$\dddot{N}$ 477·036	$\dddot{N}$ 5·76 (*l*)
Oxalic	4·5	452·875	4·51
—— crystallized	$\dddot{C}^2Aq^4$ 9·	$\dddot{C}^2Aq^2$ 78 9	$\dddot{C}^2Aq^2$ 7·88 (*m*)
Phosphoric	$\ddot{P}h$ 4·5	$\dot{P}h^2$ 892·31	$\dot{P}h^2$ 8·92
Selenious	$\ddot{S}e$ 7·	694·582	6·97
Selenic	....	$\dddot{S}e$ 794·582	7·97 (*n*)
Silica	$\dot{S}i$ 2·	$\dddot{S}i$ 577·478	$\dot{S}i$ 2· (*o*)
Succinic	6·25	627·85	6·27 (*a*)
Sulphurous	$\ddot{S}$ 4·	401·165	4·
Sulphuric	$\dddot{S}$ 5·	501·165	5·
Tartaric	8·25	834·49	8·27 (*p*)
—— crystallized	$\bar{T}Aq^1$ 9·375	....	9 4
Titanic	$\ddot{T}i$ 6·	589·092	5·95
Tungstic	$\dddot{T}s$ 18·75	1483·2	17·9 (*q*)

 (*a*.) Acetic,

(*a.*) Acetic, Succinic, &c.—The slight augmentation in the decimal is required for the increased estimate of the atom of carbon.

(*b.*) Antimonic.—Thomson's number is adopted for the simplicity of its relation to bases, in connection with the considerations in the note on antimony (*ubi supra*), but with the qualification there stated.

(*c.*) Arsenious, Arsenic.—It is difficult to read the account of Berzelius's investigations (Ann. Phil. xv. 352.) without feeling convinced by them. Those of Thomson (First Principles, i. 229) are almost equally convincing; yet both cannot be right. The sulphurets of arsenic appear, in Berzelius's paper, to consist respectively of As S^2 and As S^3, analogous to Thomson's constitution of the acids; which, having also the advantage of simplicity, in relation both to the bases with which they combine, and the oxygen they contain, seems rather entitled to preference.

(*d.*) Benzoic.—If carbon be 0·76, benzoic acid will be 15·15. But the mean between Thomson's and Berzelius's number for carbon is a fraction below 0·76, whence the number adopted.

(*e.*) Boracic.—See the note on Boron.

(*f.*) Perchloric.—Berzelius's reasons for giving this acid a different constitution from that assigned by its discoverer (First Princ. i. 85) not appearing, the latter is adopted.

(*g.*) Chloriodic, &c.—Calculated from the components in the former table.

(*h.*) Citric.—About the composition of crystallized citric acid there is some obscurity; which, though of little importance in chemistry, is otherwise in extemporaneous pharmacy, in which its neutralization is a frequent desideratum.

The most satisfactory analyses of it are those of Berzelius (Ann. Phil. v. 93.), and of Prout (Phil. Mag. and Annals, iii. 109), which nearly coincide: the former giving the crystals 17 per cent of water; the other resolving them into

Carbon............ 6 atoms.
Oxygen .... 3 atoms.
Water............ 5 atoms.

Chemists are generally agreed in making the dry acid consist of 4 atoms of carbon, 4 atoms of oxygen, and 2 atoms of hydrogen: hence Dr. Prout's statement gives 2 atoms of water to $1\frac{1}{2}$ acid; which is confirmed by Berzelius's table (*Essai*), as well as by the quantity of water above stated. But 3 atoms of acid to 4 of water is so strange a combination, as not easily to obtain our acquiescence. The equivalent weight would turn out

Acid	1	7·27
Water	$1\frac{1}{3}$..................	1·5
		8·77

Thomson

Thomson and Ure agree in assigning 2 atoms of water to 1 of acid; but the latter reduces the atomic weight to 8·375 (Dict., 2nd edit., Appendix, 84); the latter raises it to 9·5; pointing out however, in a note, its variance from his own analysis. (First Principles, i. 123, note.)

The apparent facility of determining the atomic weight of these crystals would lead us to impute the differences above quoted, to inconstancy in their water of crystallization. But having generally found this equivalent between 8·77 and 9·,

I suppose the ordinary constitution to be	Acid	1	...	7·27
	Water	$1\frac{1}{2}$	...	1·6875
And assume the equivalent 8·96.				8·9575

(*i.*) Columbic.—See the note on Columbium.

(*k.*) Ferro-prussic.—Of the discordant data relating to the atomic weight of this acid, an experiment of Dr. Ure's (Dict., 2nd edit., Appendix, 805) seemed the most direct; but subject to the uncertainty of all atomic deductions from precipitation with salts of lead, excess of which is apt to fall with the precipitate, as was observed some years since by Berzelius.

Some experiments of mine, guided by Mr. Porrett, give 13·28 for the atom of ferro-prussiate of potash; constituted as follow:

Potassium......	4·95	=	1 atom.
Oxygen.........	1·5	=	$1\frac{1}{2}$ atom.
Hydrogen.....	0·19	=	$1\frac{1}{2}$ atom.
Cyanogen.....	4·92	=	$1\frac{1}{2}$ atom.
Iron	1·72	=	$\frac{1}{2}$ atom.
	13·28		

If this be, as I believe, anhydrous ferro-prussiate of potash, the acid is 7·33: but it may be construed in other ways; and there is an anomaly about the half atoms, which becomes still more perplexing in prussian blue, composed of $1\frac{1}{2}$ atom of acid to 1 atom of red oxide of iron, or rather of 3 atoms of acid to a double atom of oxide. I had therefore some hesitation in putting the dry acid on the scale, where it is accompanied with a? The composition of the crystallized acid I have not ascertained: that of the salts corresponds in analysis (though differing in theory) with the previous determinations of Berzelius.

(*l.*) Nitrous.—It is almost superfluous to remark, that the nitrous acid of Berzelius is the hyponitrous of English chemists.

(*m.*) Oxalic.—See the note on Benzoic Acid (*d*). The crystals found by Thomson to contain 4 atoms of water (First Princ.) having occurred to no one else, seem to be the result

of

of some artifice in the manufactory, for commercial advantage, and are therefore neglected.

(*n.*) Selenic.—Discovered by Sertuerner, since the publication of Thomson's book.

(*o.*) Silica.—See note on Silicon.

(*p.*) Tartaric.—Berzelius's number contains 5 volumes, or 2½ atoms (according to our system) of hydrogen. The analyses of Thomson and Prout coincide in giving it 2 atoms, which is therefore regarded as its true composition. The augmentation in the third column is due to carbon.—See note (*b*).

(*q.*) Tungstic.—See note on Tungsten.

Having now submitted to the correction of your readers the leading equivalents, leaving the bulk to be seen on the scale itself, I wish to add a short notice of what is distinctive in the construction of the instrument.

The object was, to contain in portable compass, of easy reference, and subject to the sliding scale, all the salts and precipitates used in practical chemistry, and their secondary and elementary components; multiples of the elementary atoms occurring in vegetable and animal analysis, and of water; and to present at one view a general table of atomic weights, simple and compound, referable by moving the slider, to either the hydrogen or oxygen scale.

The symbols of Berzelius offered the compactness and facility of arrangement, which were the first requisites, with the further advantage of exhibiting the atomic construction of every substance contained; a point of some importance, as will presently appear, in apportioning compound salts, and convenient in looking out the ingredients of complicated substances, as phosphoric and hydrocyanic acids. Their deficiency, for our present purpose, was in conspicuity, which I have endeavoured to supply as follows:

Where many of the symbols had the same initial, the English is substituted in some of them for the Latin one: as T, tin; An, antimony; for Su, stannum; St, stibium; &c. &c. as explained in an index-table attached to the scale. Acids and negative bodies are distinguished by upright letters, the others being inclined; as $\dddot{\mathrm{S}}$ sulphuric acid, Cl chlorine, *Po* potash, *H* hydrogen; and Salts by combination of the upright and inclined symbols, as $Po\dddot{\mathrm{S}}$ sulphate of potash, $Ag\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{N}}$ nitrate of silver. Water (*Aq*, in running hand) attached to any symbol, shows it to be a hydrate, and generally crystallized;

*So*S Aq^{10}, sulphate of soda, crystallized, 10 atoms of water; $\bar{T}$ Aq^{1}, tartaric acid, crystallized, 1 atom of water. The simple bodies are designated in that massive letter, called by printers "Egyptian."

Oxides being also distinguished by superimpressed dots, expressing the number of atoms of oxygen, thus leave the substances without a generic sign, so reduced in number, as to make the privation almost answer the purpose of one.

The following arrangement, added to these distinctions, render this scale (with a little practice, for chemists not familiarized to the symbols) easier of reference than others much less comprehensive; so at least it is found in my own practice.

The scale is the same length as Wollaston's, but double, opening, bookwise, on hinges, and having of course two sliders, which are on the outsides. Within, is on one side a set of index-tables illustrative of the symbols; on the other, a letter-press explanation of the distinctive objects and use of the instrument.

The principal working scale is occupied entirely with the tests, salts and precipitates, arranged in four columns as follows; each column being headed with the generic symbols of its contents.

In the column left of the slider,—muriates, nitrates, borates, and a few sulphites, arsenites and silicates.

In the outer column on the left,—sulphates, arseniates, phosphates, and a few tungstates.

In the first column on the right of the slider,—carbonates, acetates, and a few chromates and chlorates.

In the outer column on the right,—tartrates, oxalates, citrates, and a few ferro-prussiates and benzoates.

Triple, quadruple and compound salts, being heavy, fall near the bottom; and their long symbols crossing two columns, readily strike the eye.

The other scale contains the simple bodies, binary compounds, and multiples, having four columns on the right of the slider, and five. On the left, each column being headed with its contents.

On the right side are negative substances, chlorides, bromides and the multiples. On the left, neutral and positive substances, and iodides.

Simple bodies are on the columns next the slider (except a few displaced to prevent crowding); oxides in the two succeeding columns to the left; and sulphurets, phosphurets, &c. in the two columns furthest left.

Acids

Acids are on the column next the slider to the right; crystallized and hydrated acids in the succeeding column; and chlorides in the two columns furthest right. Multiples of oxygen, 1 to 12, are in the second column; of hydrogen, 10 to 50, and carbon, 2 to 10, in the third; and of nitrogen, 1 to 5, and water, 1 to 25, in the outer one.

Iodides and bromides, being heavy, are all near the foot, on their respective sides; and therefore easily found, without crowding the other substances, by confining them to particular columns.

In linking from column to column, where it could be done, to prevent the embarrassment of needless multiplication of lines, precision has been allowed to give way in a few unimportant cases; but the deviation has rarely amounted to an hundredth part, particularly on the scale of salts.

The salts not packing so close as the binary compounds, and hence requiring a slider with longer degrees, an adjusting line 𝕾——𝕿 is drawn across the face on each side, to set the two scales in accordance. Thus, when a salt and a chloride are to be used together; when the binary or elementary ingredients of a precipitate are to be ascertained; or, in short, whenever the relations between salts and binary compounds or simple bodies are to be examined,—each slider must be set with the same number against the line 𝕾——𝕿, and they correspond throughout.

As a general table of equivalents, set 1·0 against O (oxygen), and the numbers are on the oxygen scale; then move the slider and bring ·8 against O, and the numbers are on the hydrogen scale, disregarding the decimal point. In the latter case the scale of salts must have its numbers correspondingly decupled.

The advantage alluded to in the outset of this notice, as belonging to the analytical character of these symbols, in cases of compound salts, may be thus illustrated. Suppose we have to prepare acetate of alumina from alum and acetate of lead: the symbol shows that alum contains four atoms of sulphuric acid; acetate of lead but one atom of oxide; and hence that four atoms of the latter salt are requisite for throwing down the acid; and that three atoms of acetate of ammonia will remain in the solution. So if we use cream of tartar, red sulphate of iron, dichloride of lime, &c. we are guided at once by the symbol what proportions to employ, accordingly as the acid or base be the subject of operation. A similar convenience is afforded in looking out the quantitative ingredients of a compound salt.

These advantages are only in promptitude; for a knowledge of

of the atomic construction of each substance is necessarily presupposed: and hence this scale is addressed less to the manufacturer or learner, than to the practical chemist.

I am, Gentlemen, yours, &c.

JOHN PRIDEAUX.

P.S.—In several scales that have fallen in my way, the stretching of the paper, in pasting, has produced important deviations from the original impression. A simple contrivance remedies this. A box tablet, the full length of the slider, and grooved to fit, is correspondingly graduated on each side of the groove. The slider is placed in this, to receive the graduated paper; which being pasted, is allowed to remain until sufficiently elongated by the humidity. It is then fixed; and such of the graduations as are out of place, by the irregular stretching of the paper, are rectified by pressure with the nail. The slider being now put in its own place, the side pieces, having been pasted at the same time, will be found nearly to correspond, and may be further stretched or shortened, and any irregularities adjusted, on comparison with an uncut impression, by the action of the nail, as before.

LXVI. *Narrative of an Excursion to the Summit of the Peak of Teneriffe on the 23rd and 24th of February 1829: with some Remarks on the Geology of that Island.* By ROBERT EDWARD ALISON, *Esq.*

[Concluded from p. 251.]

On the Geology of Teneriffe.

AT every step we take in Teneriffe, unequivocal marks appear of the great revolutions that have taken place upon its surface by volcanic action: such as craters of enormous extent and depth; conical mountains produced by eruptions; currents of lava which have flowed in every direction; beds of black and white rapilli, and tufa; with the sulphureous fumes from the Peak indicating an active state of combustion, which in a moment may be the cause of new disasters.

The lavas are of endless variety, and their appearance is variously modified, according to the heat and pressure they have been subjected to; and frequently there is a heaving-up of the strata from central points, from which they dip away in various directions as if they were elevated from the bottom of the sea by the pressure of elastic vapours, which have changed the lower strata of the island from their horizontal position to their present one of great inclination.

The lavas proceeding from the ancient volcanoes in the centre of the island, and particularly those from the Peak, may be divided into three sorts: 1st, basaltic lava of a blueish-black colour, but with an ochrey crust on the outside, very compact, and with a fracture partly conchoidal. This lava appears to be the most ancient, and is generally found near the sea; and when the stratum is thick, it frequently takes the prismatic form, but when it is rather thin, it is less compact, assumes the appearance of common trap, and is very similar to the greenstone of the Salisbury Craigs near Edinburgh:

The 2nd sort is of a grünsteinic character, of a dark green appearance: it is found in large blocks in the Cañadas del Pico, resting upon beds of pumice, which are in places eighty feet thick.

The 3rd variety is a trachytic porphyry, which forms the walls of the crater upon the top of the Peak; the colour is a brownish red, but externally it is white, from the action of sulphureous vapours upon the argil of the lava.

The other lavas, though variously modified, may be classed under two great divisions: 1st, those of a trachytic character, which are compact from being forced through the primitive ejections. And, 2ndly, lavas which are less compact, and have sometimes a vitreous and sometimes a stony appearance: these are generally covered with the last ejected masses, which are always lapilli or white rapilli.

Frequently the first stratum of the modern lavas, reckoning from below, is a dull trachytic porphyry, covered by rapilli or an earthy conglomerate which alternate several times. The next is generally a cellular augitic lava, containing felspar, more or less decomposed, alternating the same way as the others: and last of all, at the surface is a sort of basaltic trap (similar to what is called whinstone in Scotland) of a deep black colour, but where it is exposed to the atmosphere it is covered with a yellowish-coloured rind. Most of the compact lavas strongly affect the magnetic needle, from the quantity of titaniferous iron they contain.

Upon the sea-coast, about five miles to the west of Orotava, are basalts of a regular hexagonal figure; and near the same spot, at an elevation of about 120 feet above the sea, there is a thick bed of clayey volcanic mud, containing quantities of marine shells. It is difficult to account for this phænomenon, otherwise than by supposing that they were drawn into the crater of the volcano through some crevices between it and the sea, and afterwards thrown out with the mud. It is well known, that many of the existing volcanoes have a communication with

with the sea. Humboldt mentions that some of the Andes frequently throw out vast quantities of water, and sometimes mixed with fish. In 1824, a volcano in Lanzerote sent forth a large body of salt water, which did considerable damage to the surrounding lands.

Columnar lava is found not only near the coast, but likewise in several ravines at considerable elevations, and even in the Cañadas del Pico, which are eight thousand feet above the level of the sea. I have particularly observed that whenever the stratum of lava is thick, it appears to have a constant tendency to take the prismatic form; and little difference appears to exist, whether it cooled quickly or slowly, as the basalt near the sea, proceeding from a crater four or five miles off, and flowing over an inclined plane of five degrees and a half, is equally crystallized [?] as the basalts from a crater only a few hundred yards off. Occasionally the lower part of the stratum is perfectly crystallized [?], whilst the upper surface is without the slightest sign of crystallization [?], and only presents a close and compact mass. These basalts are internally of a blueish slate-colour or a black, sonorous, compact and hard, and frequently include small crystals of greenish augite, and leucite of a vitreous lustre.

The columns are generally from one to two feet in breadth, sharply defined, and frequently destitute of articulations; very few of them are straight, but are bent in the middle like the ribs of a ship, as if unable while in their soft state to support the mass of incumbent matter. They appear to decompose quickly by the action of the atmosphere; and from the iron they contain, all the prisms are covered by a yellowish-coloured coating.

There is a species of basaltic trap exactly similar in composition to the columnar basalt; but I do not recollect having seen it in thick strata, but generally traversing volcanic breccias in dykes of four or five feet in thickness, nearly perpendicular to the horizon. These aggregates contain a mass of rounded pieces of lava, strongly calcined, and large crystals of hornblende; the basis or cement is generally a tufa or brown mud.

It is worthy of observation, that these dykes are generally found nearly at the same elevation; that is, from four to five thousand feet above the level of the sea; occasionally they are to be met with higher, but seldom lower.

In the south-east side of the valley of Orotava, at a place called los Organos, is the crater of an extinct volcano, which evidently has fallen in and formed a small but picturesque valley called Agua Mansa: one side of the crater still remains,

 and

and presents a perpendicular wall of aggregates 150 feet high; this wall is crossed at right angles by dykes of trap, which at a distance give it the appearance of the pipes of an organ.

Near this spot are large scattered masses of amorphous amygdaloidal lava, filled with crystals of augite, hornblende, idocrase, leucite, felspar and cubicite.

On the west side of the island, in the valley of Vilna, there are high dykes of trap: the space between these dykes was evidently filled up with breccias at one period, but now they are destroyed (probably by torrents of water from some of the neighbouring volcanoes), leaving isolated walls of trap, which the ignorant natives suppose were erected by the Guanches, or some supernatural beings. The basaltic dykes do not (like those in Scotland and other places) follow a particular point of the compass, but they are flexuous and uncertain.

The composition of lava is not only extremely various from the same volcano, but from different parts of the same stream: near the crater it will be close, compact, and free from crystals; but if it be traced to some distance below, it will be found frequently to be vesicular, and to contain numerous crystals.

The lavas are generally in broad streams, from three to twenty feet thick; but large amorphous blocks are frequently met with, containing numerous crystals of augite, felspar and hornblende, and occasionally olivine.

Upon some of the small volcanoes called in the country Montañetas, are found hollow hemispherical masses, which I name, from their appearance, volcanic bombs; they are generally twelve inches in diameter: the exterior is a compact reddish-coloured lava, but the interior is much less compact, as if following the law of fluid bodies when turning round a centre. They are covered generally on the outside by a white rind, probably caused by the action of sulphureous vapours. Some of the bombs are composed of obsidian to a thickness of three or four inches from the outside, but nearer the interior they present a fibrous appearance showing its passage into pumice. The montañetas are covered with irregular detached pieces of scoriæ and volcanic slag; they are all much tumefied and expanded by sulphureous vapours, and almost as light as pumice. These slags frequently exhibit all the colours of the rainbow, but their general appearance is a dark blue, a black, or a brown, and some so exactly resemble blue iron slag that it is difficult to distinguish them from it.

There are several varieties of pumice in different parts of the island.—The 1st, and most common, is of a grayish or a grayish-white colour, occasionally tinged with red. The 2nd is

is of the same colour, but has a porphyritic character, as it contains crystals of augite and felspar:—these two varieties are in the greatest abundance in the Cañadas del Pico, which in many places are covered to the depth of seventy or eighty feet. The 3rd variety is of a pale olive colour; its surface is rough, but the pores are minute. The 4th is a gray pumice with small veins of carbonate of lime traversing it. The 5th variety hardly ought to be called pumice, as it is a vitreous obsidian which has had the outside changed by the great strength of volcanic heat into a dull rough sort of pumice, with the fibres hardly discernible.

The 3rd, 4th, and 5th varieties of pumice, I have only seen in the neighbourhood of the Peak; but no doubt they are to be found in other places, as pumice entirely covers most of the plateaux or table-lands of the island (which are only extinct craters of volcanoes, several square miles in extent); and when the mountain torrents break through the lavas, and form ravines of an enormous depth, beds of pumice are frequently observed of great thickness. The finest specimens of pumice are to be seen upon a mountain behind the town of Guimar, and I understand they are equal, if not superior, to those of the Lipari islands.

It appears that the white cinders are thrown out last from a volcano, and announce the end of an eruption. When the heavy compact lavas had ceased flowing, the black rapilli were ejected; after which, the volcanic fires still diminishing in strength, the white rapilli were then thrown out, but to a smaller distance.

This last-named production surrounds the Peak, and covers vast plains some miles from it: but from other volcanoes it seldom extends so far as the sea, nor to a great distance from a crater. The black rapilli, on the contrary, are found at a considerable distance from any crater, and frequently in thick strata near the sea.

There are several sorts of tufa, and they generally contain a large proportion of carbonate of lime. One sort is generally found in rather thin beds under numerous strata of lava; it is a brick-red colour, and may be called a puzzalana, as it consists of a carbonate of lime with silex and a large portion of iron, and to have flowed in a perfectly fluid state, and turned into a brittle red mass by a stream of hot lava afterwards flowing over it. I have repeatedly observed, that when the stratum of lava over it was thicker in one place than in another, the tufa was of the deepest colour, where it had been exposed to the greatest and longest continued heat. The second variety resembles trass or tarras, and is found in similar

milar situations to the first; the colour is yellow and hard, but it is brittle, and rough to the touch. The third sort, called by the inhabitants *Tosca*, is a species of piperino; the colour is a dirty white, and occurs in beds varying in thickness from twelve inches to twelve feet: it is only a carbonate of lime mixed with white rapilli. It is sufficiently tenacious for building purposes, and is used for the upper part of walls and terraces. The fourth variety is of a dark-gray colour, or almost approaching to a brown, and is used by the inhabitants for the formation of filtering stones. At Candelaria, about eight miles from Santa Cruz, it is found in great abundance, and superior to any stones for the same purpose that I have seen in England. The price being only a dollar or a dollar and a half for one of large dimensions, it is an appendage to every house; the form of them is a rectangular parallelopiped surmounting a hemisphere, whose diameter is equal to the side of the square which serves for the base; they are placed in a wooden frame surrounded by lattice-work to keep the water cool which is filtering.

Two or three varieties of obsidian are to be observed in Teneriffe. The first is in immense blocks weighing from forty to one hundred tons; they are to be found in the Cañadas near the foot of the Peak, at an elevation of 8100 feet above the sea, and appear to have been thrown out by the last eruptions; they generally approach a spherical form, and many of them are split or broken into fragments by their fall. The colour is a greenish-black, with a chatoyant lustre; the fracture is irregular or hackly, and separates by a slight blow: it frequently presents on the outside a fibrous appearance, denoting its passage into pumice, and it generally contains large white crystals of semi-vitrified felspar. The second variety is of a very opposite character: the colour is a jet black, possessing internally a shining vitreous lustre; it is hard and compact, breaking with a conchoidal fracture, and is translucent at the edges. The inhabitants call it *Tobona*, which was the name given it by the Guanches or ancient inhabitants, who formed all their cutting instruments of it. It is sometimes found in streams, which in cooling have formed large detached blocks, and in other situations it appears in long continued currents: there is one of considerable thickness extending from the north side of the Peak to the district of La Guancha, in the valley of Icod, a distance of nine or ten miles. The third variety has all the qualities of the second, but it is of a dark-green colour. I have only found it in small pieces.

TABLE

Table of the Temperatures observed on the Peak of Teneriffe on the 23rd and 24th of February 1828; with the Temperature at corresponding periods in the Port and Town of Orotava.

Hour.		Therm.		Therm.			Therm.
12	Port of Orotava	67°	Town of Orotava, 1141 feet high	59°	Foot of the Peak	8,957 feet high	44°
23rd. 1 *p.m.*	Do.	No observat.	Do.	59·75	Estancia	9,930 *	50
2	Do.		Do.	60	Alta Vista Arriba	10,621	43·875
3 30m	Do.		Do.	60	Estancia	9,930	42
4 15	Do.		Do.	60	Do.	Do.	42·25
5	Do.		Do.	59·5	Do.	Do.	36·5
5 45	Do.	65·	Do.	59	Do.	Do.	36
7	Do.	63·	Do.	58	Do.	Do.	36
8	Do.	63·	Do.	57·5	Do.	Do.	34
9	Do.	62·	Do.	56·5	Do.	Do.	38
10	Do.	61·5	Do.	55·5	Do.	Do.	39
11	Do.	No observation.	Do.	55	Do.	Do.	39
12	Do.		Do.	No observation.	Do.	Do.	38·5
12 15	Do.		Do.		Do.	Do.	38·5
24th. 1 *a.m.*	Do.		Do.		Do.	Do.	38·5
2	Do.		Do.		Do.	Do.	38·5
2 45m	Do.		Do.		Do.	Do.	38
3 15	Do.		Do.		Do.	Do.	37·5
4	Do.		Do.		Do.	Do.	37
5 30	Do.		Do.		Do.	Do.	36·5
6	Do.	57·5	Do.	50·5	Alta Vista Arriba	10,621 feet high.	39
7	Do.	59·	Do.	51·25	Cave of Ice	11,098	42·75
8	Do.	61·	Do.	52·875	Rembleta.	11,721	46
9	Do.	61·5	Do.	54·25	Summit of the Peak	12,188	45·875
12	Do.	64	Do.	57·5	Estancia.	9,930 †	76

* The clouds were resting on the Peak, and the wind was very high.

† The sun hot, and no wind.

TABLE of the Decrease of Temperature, from the Port of Orotava to the top of the Peak of Teneriffe; with the supposed Heat at different Stations, if the Decrease were in Geometrical or Arithmetical Progression (from Observations made February 23rd and 24th, 1828).

Hour of Observation.	Stations.			Elevation of the 2nd Station above the 1st.	Elevation of the last above the 2nd.	Temperature of the 1st Station.	Temperature of the 2nd Station.	Temperature of the last Station.	No. of Feet of ascent for an alteration of 1° Fahr. from 1st to 2nd Station.	No. of Feet of ascent for an alteration of 1° Fahr. from the 2nd to the last Station.	Calculat. Temperature of last Station, if the decrease were in Geometrical progression.	Calculat. Temperature of last Station, if decrease were in Arithmetical progression.
	1st Station.	*2nd Station.*	*Last Station.*	Feet.	Feet.	°	°	°	Feet.	Feet.	°	°
1 *p.m.*	Port of Orotava.	Town of Orotava.	Estanciade los Ingleses	1141	8789	67	59·75	50	142·5	900·41	24·72	+ 3·904
6	Do.	Do.	Do.	...	...	65	58	36	163	399·5	23·55	+ 4·08
7	Do.	Do.	Do.	...	...	63	57·5	34	207·45	369·74	28·38	+15·1
8	Do.	Do.	Do.	...	...	63	56·5	38	175·53	475	24·45	+ 6·43
9	Do.	Do.	Do.	...	...	62	55·5	39	175·53	532·6	23·68	+ 5·43
10	Do.	Do.	Do.	...	...	61·5	55	39	175·53	549·25	23·27	+ 4·93
7 *a.m.*	Do.	Do.	Cueva de Nieve...	...	9940	59	51·25	42·75	147·22	1169·41	15·03	—16·26
8	Do.	Do.	Remblets ...	...	10535	61	52·75	46	138·3	1560·74	13·19	—23·42
9	Do.	Do.	Summit of the Peak	...	11047	61·5	54·25	45·875	182·05	1317·85	16·08	—15·94

A TABLE of the Temperature at different points of Ascent, observed in a Journey to the Top of the Peak of Teneriffe, made February 23rd and 24th 1828; with the number of feet ascended, for a decrease of Heat equal to one degree of Fahrenheit.

	Stations.	Height of the Column of Air.	Tempera-ture at the Lower Station.	Tempera-ture at the Upper Station.	Number of Feet ascended for a Decre-ment of Heat equal to 1°.
		Feet.	°	°	Feet.
Feb. 23. 7 *a.m.*	Upper part of the Town of Orotava, and Pino del Dornajito	2·269	56·5	51·5	453·8
7 30m	Do. Do. to about	2·559	56·25	50·5	445·04
8	Do. Do. Do.	2·840	56·25	49·5	420·74
9	Do. Do. Do.	3·400	57·5	49	400
9 30m	Do. Do. Do.	3·660	58	50·25	472·358
10	Do. Do. near the Portillo	3·997	58·5	51	530·266
10 30m	Do. Do. to about	4·840	58·875	50·5	579·104
11	Do. Do. Do.	5·700	59	47	475
11 30m	Do. Do. Do.	6·560	59·125	46	499·809
12	Do. Do. to foot of the Peak	7·817	59	44	511·135
24. 2 *p.m.*	Do. Do. near the Portillo	3·997	57·75	52	517·721

LXVII. *On determining the Longitude by Occultations of the fixed Stars.* By Mr. Thomas Squire.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

OCCULTATIONS of the fixed stars by the moon are phænomena that would seem to offer the best means for determining the longitude of places on the earth's surface of any yet known, at least as far as regards observation; as the instant of immersion or emersion can mostly be obtained to the fraction of a second. But, on the other hand, the computations which are necessary for obtaining the desired results may, under certain circumstances, require data that are not so well established as the nature of the problem demands; and therefore, though the observations may be taken with the greatest care, yet the longitudes thence obtained may not always prove so satisfactory as the known accuracy of the observations might lead us to expect, even when the process of computation is managed with the greatest circumspection.

In confirmation of the above remarks the occultation of Aldebaran, on the 15th of October 1829, may be very properly cited as an example. The weather proving favourable at the time of this phænomenon, both the immersion and emersion of the star were accurately observed at Greenwich and at Epping; and from these observations computations have been made upon two suppositions, with the view of obtaining the respective longitudes. First, by considering the effects of parallax as computed from the altitude of the star, at the same time using the moon's semidiameter without augmentation for altitude, and the orbital angle as given in the elements. Secondly, by using the parallactic depression as found from the apparent zenith distance of the moon's centre, also her visible semidiameter, and the correct orbital angle as found for the times of immersion and emersion.

Hence, according to the first rule the immersion gives the longitude of Greenwich = 0^s, as it ought to do; and that of Epping = $25^s{\cdot}37$ E., which is also very near the truth; but the emersion makes the longitude of the former place = $23^s{\cdot}34$ E! and that of the latter = $49^s{\cdot}34$ E!

Again, by the second rule the longitude of Greenwich = $0^s{\cdot}8$ E., and that of Epping = $25^s{\cdot}92$ E., according to the times of immersion; but by emersion the longitude of Greenwich = $23^s{\cdot}28$ E! and that of Epping = $49^s{\cdot}49$ E! which numbers agree very nearly with the above.

Here then we see that the results according to both methods are correct, or very nearly so, for immersion, yet greatly in

in excess for emersion, which might lead us to suppose that in the latter case there is some error in the observations; but as we cannot suspect this to be the case at the Royal Observatory, and the discrepancy being nearly the same at both places, it may reasonably be inferred that the time of emersion at Epping is equally correct as that at Greenwich. And, moreover, as both methods of computation are founded upon true mathematical principles, it were for these reasons naturally to be expected that the results for the respective places would have been the same at emersion as at immersion. But as that is not the case, some natural cause must have operated in producing an effect in the former instance that did not take place in the latter, and which, being unknown, could not enter as a compensating quantity into the elements of computation.

Before any opinion is hazarded on this point, it may be proper to observe, that in this occultation the immersion took place at the light, and the emersion at the dark border of the moon. Now it is well known that the rays of light suffer a degree of inflection when they pass near the surface of an opaque body. Hence in this case, when a direct ray of light from the star became a tangent to the dark limb of the moon, which was the absolute time of emersion, it was bent towards that body, and thence thrown off at some distance from the spectator; so that the moon had to advance a few seconds in its orbit before the star could be seen by the observer.

It is pretty evident that some phænomenon of this kind must have taken place at the emersion by which the occultation was retarded several seconds beyond what the true semidiameter of the moon, and her visible horary motion, would give. If therefore we increase the moon's radius a small quantity (in the present instance $9''\cdot74$), as a compensation for the time it took for her western limb to reach the incidental point of inflection, then we shall have the longitudes of both places as correct at emersion as at immersion*.

Perhaps, after all, the above may not be considered an adequate solution of the difficulty:—if so, I can only say that I have nothing better to offer just now; and therefore hope some of your scientific readers will have the goodness to give their opinions on the subject, by pointing out the reason why correct observations of the immersion and emersion of a star at any place should not give the longitude the same, with the same method of computation founded upon a true mathematical basis. I have not thought it necessary to enter minutely into the method of solution, as that must be evident to most

* Some observations on the value of the *Moon's inflection* will be found in No. 29 of the Proceedings of the Astronomical Society, p. 190.—Edit.

who

who are at all acquainted with investigations of this nature. A polar compression of $\frac{1}{300}$ has been adopted in the computations; and the logarithms, &c. have been carried to seven places of decimals. I am, Gentlemen, yours, &c.

Epping, Nov. 16th, 1830. THOMAS SQUIRE.

LXVIII. *Notice of the Arrival of Twenty-six of the Summer Birds of Passage in the Neighbourhood of Carlisle, together with some of the scarcer Species that have been met with in the same Vicinity during the Year* 1830; *with Observations, &c. By* A CORRESPONDENT.

No.	English Specific Names.	Latin Generic and Specific Names.	When first observed.		No.
1	Quail	Coturnix vulgaris	May	7	6
2	Swallow	Hirundo rustica	April	6	35
3	House Martin	——— urbica	——	26	36
4	Sand Martin	——— riparia	March	29	36
5	Swift	Cypselus Apus	April	28	37
6	Goatsucker	Caprimulgus europæus	May	1	38
7	Pied Flycatcher	Muscicapa atricapilla	April	23	41
8	Spotted Flycatcher	——— Grisola	——	28	42
9	Ring Ouzel	Turdus torquatus	——	22	49
10	Wheatear	Saxicola Œnanthe	——	1	53
11	Whinchat	——— rubetra	——	23	54
12	Redstart	Sylvia Phœnicurus	——	10	57
13	Grasshopper Warbler	Curruca Locustella	——	8	58
14	Sedge Warbler	——— salicaria	——	28	59
15	Greater Pettychaps	——— hortensis	May	2	62
16	Wood Wren	——— sibilatrix	April	28	63
17	Blackcap	——— atricapilla	——	10	64
18	Whitethroat	——— Sylvia	——	27	66
19	Yellow Wren	Regulus Trochilus	——	10	70
20	Yellow Wagtail	Motacilla flava	——	12	75
21	Field Lark or Titling	Anthus trivialis	——	18	78
22	Cuckoo	Cuculus canorus	——	23	121
23	Wryneck	Yunx Torquilla	——	13	125
24	Corncrake or Land-Rail	Ortygometra Crex	——	23	129
25	Dottrel	Charadrius Morinellus	May	12	164
26	Common Tern	Sterna Hirundo	April	29	235

Note.—The figures contained in the column on the right in the above Table, as well as those affixed to the species not included in it, refer to the numbers in Fleming's History of British Animals, which we have inserted, in order that the reader who wishes to have a description, or see the various synonyms of any of the birds here alluded to, may find the species at once, should he possess that highly useful and most excellent work.

Swallow.

Swallow.—On referring to our remarks upon this species, it will be seen that we considered the arrival of the Swallow near Carlisle, last year, unusually early*. We have, however, to record its appearance this year three days sooner, namely on the 6th of April. Three or four were seen on the 10th, exactly in the same situation as the one observed last year on the 9th of April.

Sand Martin.—This is the first time we have been able to see the Sand Martin arrive in March since we have paid any attention to the appearance of the summer birds of passage in this neighbourhood. In the year 1788 it was observed nearly in the same locality as early as the 25th of this month.

Goatsucker.—We have this year been so fortunate as to obtain six eggs of the Goatsucker, four on the 2nd, and two on the 6th of June. These were all found exposed on the bare ground, and so much resembled the small grey or whitish stones commonly found on heaths and mosses, that in one instance they were found with great difficulty, notwithstanding the female was put off her eggs.

The eggs of this species are rather elegantly formed, some being almost perfectly oval; they however vary much in size, weighing from 112 to 134 grains.

Pied Flycatcher.—Either the Pied Flycatcher was later than usual in making its appearance this year, or it had escaped our search; for we could not discover it before the 23rd of April. A pair had a nest in the identical hole where this species has bred for four successive years. On the 14th of May this nest contained eight eggs, arranged in the following manner: one lay at the bottom, and the remainder were all regularly placed perpendicularly round the sides of the nest, with the smaller ends resting upon it, the effect of which was exceedingly beautiful.

We regret to observe that we were not successful in our attempts to rear the two young Pied Flycatchers mentioned in our last communication†. We are satisfied that our want of success may be attributed, in a great measure, to allowing them to wash too frequently when first caged, from the effects of which they never completely recovered. The first died on the 24th of September, the other survived until the 20th of October.

After the beginning of September the strongest bird became exceedingly restless and uneasy, especially in the evening and at night; and occasionally to such a degree, that the person who had the care of it was more than once induced to get up in the night, being apprehensive that it had been attacked by a cat. It would frequently start up suddenly from the perch,

* Philosophical Magazine and Annals, N. S. vol. vi. p. 276. † Ibid. 278.

dart

dart with great violence against the wires of the cage, flutter about in the strongest manner, and was often so much exhausted with these efforts, that the whole frame was convulsed; it panted, gasped, and was to all appearance in the very agonies of death. All these symptoms are we believe common to most migratory birds when first kept in confinement, and are a strong proof that this species migrates, notwithstanding the assertion to the contrary by Montagu and some other authors. The plumage underwent a material change: it gradually lost the dusty speckled feathers peculiar to the majority of young birds, and had nearly acquired the livery of the female. It proved however to be a male.

Ring Ouzel.—We could not ascertain that the Ring Ouzel was seen in its usual breeding haunts before the 22nd of April. Yet we think it probable it arrived sooner, having secured two young birds in full feather on the 15th of June. The last was noticed on the 28th of September.

123. *Greater Spotted Woodpecker* (*Picus major.*)—One of these birds was shot near Croglin on the 8th of September; it is a species very rarely seen in the neighbourhood. The last was caught alive by a cat a few years ago in a garden in the suburbs; and having received little or no injury, it amused the curious for some days, by almost incessantly tapping with its bill the wooden bars of the cage in which it was confined.

131. *Spotted Gallinule* or *Rail* (*Gallinula Porzana*).—Two specimens of this prettily marked species have lately fallen under our observation. The first was killed on Hayton Moss on the 9th of September, the second near Jedburgh in Scotland a week afterwards. Montagu, in his Ornithological Dictionary, makes the following observation upon this species:—"In England it has not been observed further north than Cumberland." Whether this is the first Spotted Gallinule that has been detected in Scotland we know not; yet it is probable it may have occurred there since the publication of the Ornithological Dictionary in the year 1802.

133. *Grey Phalarope* (*Phalaropus lobatus*).—The first Grey Phalarope we believe ever recognized in this part of the county, was killed on the coast not far from Cardurnock on the 18th of September. This bird had a small clay-coloured patch on each side of the neck near the middle. The sex could not be ascertained by dissection, owing to the injury it had received. There was nothing in the stomach except the fragments of a few shrimps (*Crangon vulgaris*), and it only weighed one ounce one dram.

This is undoubtedly a rare species, especially in the northern counties; but it would appear from the annexed quotation

tation, extracted from the Magazine of Natural History *, that it is not considered by any means rare in some parts of Cornwall. "Not uncommon on the coast in winter, but their habits make them seem so rare. They never perch on rocks or the sands, but alight on the water with ease, and are capable of swimming against a rapid tide. Not shy."

138. *Dusky Sandpiper* (*Totanus fuscus*).—A specimen of this very elegant Sandpiper was shot in Solway Firth near Bowness on the 13th of October. It was a female; the stomach was empty, and it barely weighed four ounces four drams. The plumage agreed in almost every respect with the figure and description of the Spotted Snipe, given by Montagu in the Supplement to the Ornithological Dictionary; which, according to Temminck, is a young bird of the year.

This is unquestionably one of the rarest of the British Sandpipers, and in very few cabinets. Pennant records a solitary specimen killed in Anglesea; Bewick two in the north; two others came into the possession of Montagu, both taken in Devonshire; and the authors of the Catalogue of the Norfolk and Suffolk Birds mention four more,—three shot in the vicinity of Yarmouth, the fourth near Ipswich †. It appears also to have occurred on the coast near Whitehaven ‡.

139. *Redshank* (*Totanus Calidris*).—Three or four Redshanks were shot on Brugh Marsh on the 20th of August. This species used formerly to be considered somewhat rare in this neighbourhood; but during the last few years several have come under our inspection.

140. *Green Sandpiper* (*Totanus ochropus*).—A specimen of this elegant bird was killed near Walby on the 1st of September, which is the third we have seen within a few years.

156. *Knot* (*Tringa Canutus*). Three Knots were shot on the coast on the 9th or 10th of September, and are we believe the first birds of this species that have been detected in the neighbourhood of Carlisle.

158. *Ruff* (*Tringa pugnax*).—During the month of September five or six of these birds were obtained, one near Park House, a second on the coast, and two others not far from Kirklinton. There was little or no variety in their plumage, being all females or young birds.

We cannot find that the Ruff has been noticed in this part of the county before.

159. *Turnstone* (*Strepsilas Interpres*).—A small flock of

* vol. iii. p. 177.
† Transactions of the Linnean Society, vol. xv. p. 44.
‡ Magazine of Natural History, vol. iii. p. 171.

Turn-

Turnstones were occasionally seen on the coast between Bowness and Cardurnock from the middle of May to the beginning of June. A young female was killed on the 13th of the former, and an old male nearly in full summer plumage on the 3rd of the latter month. This is only the second instance of the Turnstone having been met with in Cumberland that we are aware of; the first was killed on the borders of Ulleswater on the 11th of May 1801.

161. *Grey Plover* (*Squatarola cinerea*).—Four Grey Plovers were procured on the coast in the month of September. Diminutive as the hind toe of this species is, it varies much; in some specimens it is scarcely $\frac{1}{10}$th, while in others it exceeds $\frac{2}{10}$ths of an inch in length. The Grey Plover is by no means common on the shores of Solway Firth, although there can be little doubt that it is frequently confounded with the Golden Plover (*Charadrius pluvialis*) by persons not conversant with ornithology. It may be, however, always known with facility by the long black feathers beneath the wing at the base; or, as Montagu calls them, the inner scapulars, which form an excellent specific distinction.

Dottrel.—On the 12th of May a gentleman residing in the city, when searching for Corncrakes, accidentally fell in with a small flock of Dottrels near Kingmoor House, a short distance from Carlisle; two of which he shot. This occurrence had no sooner transpired, than the remainder were almost immediately pursued, and we have reason to believe that not one escaped. Ten came under our inspection; the majority were young birds, and not one had acquired the perfect plumage of the adults in summer. A single specimen was shot on Cardurnock Moss about the middle of August, and another was killed on Cross Fell on the 18th of September.

The spring of this year was remarkably early, and when compared with the preceding, the vegetation in general was fully three weeks in advance. The temperature of the weather during the last week in March and throughout the greater part of April was unusually mild, warm, and genial. This, however, was succeeded by one of the wettest summers almost ever recollected in this district. It will be seen upon inspecting the subjoined Table, that from the 1st of May to the 30th of September, an interval of one hundred and fifty-three days, there was more or less rain on one hundred and seventeen days, leaving only thirty-six fair days, or little more than one month out of the five.

Several

Several of the birds of passage arrived unprecedentedly earl in the north, particularly the Swallow, Spotted Flycatcher, Grasshopper Warbler, Blackcap, &c.

A Table showing the number of Days on which Rain fell more or less at Carlisle, from the 1*st of May to the* 30*th of September* 1830.

Months.	Days.	Number of Days on which there was much Rain.	Number of Days on which there were occasional Showers.	Number of Days on which there were slight Showers or a few drops of Rain.	Number of Wet Days in each Month.
May............	31	5	8	9	22
June...........	30	8	8	6	22
July...........	31	6	10	6	22
August........	31	9	14	3	26
September....	30	13	7	5	25
	153	41	47	29	117

Carlisle, November 1st, 1830.

LXIX. *Notices respecting New Books.*

Elements of Practical Chemistry, comprising a Series of Experiments in every Department of Chemistry, &c. By DAVID BOSWELL REID, *Experimental Assistant to Professor Hope, &c. &c.*

THIS work is presented to the chemical student under circumstances calculated to raise high expectations of its value. Mr. Reid was formerly the pupil, and is now the chemical assistant, of Dr. Hope,—a lecturer, who is justly celebrated for the excellence and accuracy of his experimental illustrations: to him the book is dedicated, and we may, perhaps, not unfairly presume that it did not make its appearance entirely without his sanction.

Thus advantageously situated, Mr. Reid must excuse us, if we examine his pretensions, which are by no means slight, with some degree of minuteness. "The object of this work," says the author, "is to present the student with a systematic series of experiments, sufficiently broad to lay a proper foundation for acquiring habits of practical skill in chemical operations, with precise and minute directions for enabling him to perform them."

We shall not pay any attention to Mr. Reid's arrangement, nor proceed regularly through the work, but confine our remarks principally to the chapter on Nitric Acid. The subject is one of import-

 ance;

ance; and as it involves in our opinion no peculiar difficulty, either theoretical or practical, we trust we shall be considered as acting candidly towards the author in selecting it for examination.

To prepare nitric acid, Mr. Reid directs the use of equal weights of nitre and sulphuric acid, and recommends that the acid distilled should be condensed in a receiver kept cold by a slender stream of water. Now although in this operation the receiver certainly becomes slightly warm, yet it is never rendered hot; and we know from direct experiment that within $\frac{1}{100}$ of the whole quantity of acid contained in the nitre may be procured without the aid of any artificial cooling whatever; when, however, it is required, the plan of placing the receiver in a vessel of cold water is much more simple and easy of execution, and requires much less attention than the mode of cooling recommended by Mr. Reid. In the figure representing the apparatus a flask is employed instead of a proper receiver: now this, on account of its form, must render the apparatus liable to accident from unsteadiness; a common bottle is much better, but a tubulated receiver used with it is greatly to be preferred.

"The distillation" of nitric acid, says Mr. Reid, p. 54, "may also be conducted in flasks with a long glass tube bent at one end in the manner shown in the figure, or the condensation of the acid may be effected almost entirely in the tube. This is a very convenient method of conducting the process, and is often preferred to distilling the nitric acid from a retort, though beginners find some difficulty in adjusting the tube. The nitre and the sulphuric acid are first put into the flask, and a thin tube bent at an acute angle, about two inches from one extremity, and a very little less in diameter than the neck of the flask, is surrounded with some well-worked clay, and put into it, a small quantity of plaster of Paris being placed over the luting to render it tight."

The reason for this apparatus being "often preferred," Mr. Reid has not given; nor can we discover any, unless that which is confessedly difficult to beginners is peculiarly easy to the initiated. We do however assert, without having tried, and without intending to attempt this process, that it is the most inconvenient which it is possible to devise; a flask is much less proper than a retort, as will occur to any one who will for a moment compare the diameters of the descending tube of the former, with the descending aperture of the neck of the latter: owing to the narrowness of the tube, much nitric acid which condenses, necessarily runs back into the flask, and consequently the process is lengthened. It must be difficult, not only to the beginner but also to the veteran, to fasten the glass tube into the flask with clay; and to render it secure two lutes are recommended, when in common cases one is sufficient; a thin tube when bent is extremely apt to break at the angle, more especially when it is to be fastened into two flasks, one of which is unsteadily supported on a ring, and the other equally so on its side.

In explaining the nature of the action occurring between sulphuric acid and nitre, Mr. Reid observes: "Every two equivalents of

of the common sulphuric acid (96) consist of two equivalents of water (18), and two of dry sulphuric acid (80); the nitrate of potash on the other hand, is composed of one equivalent of potash (48), and one of nitric acid (54). The dry sulphuric acid combines with the potash, forming bisulphate of potash, and the water goes to the nitric acid, forming the liquid which is condensed in the receiver:" and we are afterwards informed (p. 56) that the nitric acid of the nitre requires two equivalent portions of water to condense it. When we first noticed that according to the above statement, 18 added to 80, are equal to only 96, we considered it as an error of the press; but in the following page the same blunder is more conspicuously repeated in a diagram; we must, therefore, charge Mr. Reid with an extreme want of attention. When corrected we admit that this is the view of the subject taken by the late Dr. Wollaston; but it is not an accurate one, as Mr. Reid might have seen in the second volume of the Phil. Mag. and Annals of Philosophy, p. 430. It is *not* the dry sulphuric acid only which combines with the potash, the bisulphate always contains water, which is essential to its exstence; consequently the whole of the water does *not* go to the nitric acid, and two equivalents of water are *not* required to condense one equivalent of nitric acid.

Indeed Mr. Reid has given two tables of the strength of liquid nitric acid of different densities; and if he had examined them, one of two things must have occurred to him: viz. either that his assertion, as to the smallest quantity of water which is capable of condensing a given proportion of nitric acid, was erroneous; or that these tables were both extremely inaccurate. The first of them is Dr. Thomson's; it shows not only the per-centage of real acid, but also the atomic constitution; and it surely ought to have excited Mr. Reid's notice, that according to this table it is acid of sp. gr. 1·4855, which contains 75 per cent of real acid, and which is stated to be constituted of 1 atom of acid, and 2 atoms of water; whereas Mr. Reid asserts that this is the composition and atomic constitution of acid of sp. gr. 1·5. The second table is Dr. Ure's; and if he had doubted the accuracy of Dr. Thomson's statement, he would have found its correctness confirmed by referring to Dr. Ure, who mentions that acid of sp. gr. 1·485 contains 74·918 per cent of real acid.

It is quite evident that Mr. Reid never compared these things, or he would not have considered that statements so contradictory, as that a table founded on the assumption that acid of 1·4855 contains 75 per cent of acid, would "be found very convenient to refer to in making experiments," when he had previously asserted that acid of sp. gr. 1·500 was of that strength.

After having mentioned the weights of nitre and sulphuric acid proper to be employed, and adverted to the circumstances attending the use of one atom each of the acid and salt, we did not expect that our author would have given further instructions on the subject; but to our surprise the following directions occur at p. 56: "in conducting this process on the small scale, three ounces (water) measure

measure) of sulphuric acid to eight ounces by weight of nitre will be found convenient proportions." In this short quotation there is much to perplex a student; but "precise and minute directions" for enabling him to perform the experiment are lamentably deficient. The quantities which had been previously mentioned, we supposed were intended for operations on the small scale, since flasks are the vessels in which they are directed to be conducted; and we have heard of wine measure, beer measure, and imperial measure, but we have yet to learn what water measure is. An ounce water measure may, we conceive, have several meanings; first, the measure of an avoirdupois ounce of water; secondly, the fluid ounce used in the London Pharmacopœia; and, thirdly, the measure of a troy ounce of water: so that the student has the chance of using a measure equal to 480, 454·5, or 437·5 grains of water. Nor is this all; he is directed to take eight ounces of nitre, and "by weight," for fear he should measure them; but as to whether the ounces are to be avoirdupois or troy weight, he is totally without a guide. As however the quantities, whatever they may be, are specially directed for the "small scale," the student may suppose that they are to be as different as he can make them by the laxity of the rule, from the proportions previously assigned: in this case he may take the avoirdupois ounce of water for the measure of the acid, and the troy ounce in weighing the nitre; and then the weight of the acid will be to that of the nitre about as 2 to 3·16, which are indeed very nearly the quantities given in the Edinburgh Pharmacopœia, and proportions less in unison with the doctrine of equivalents could hardly be adopted: on one plan of operation the quantity of sulphuric acid is too great, and that of the water which it contains is too small on the other.

Referring to the red acid, which is a mixture of nitric and nitrous acids, Mr. Reid says: "It is seldom necessary to expose the mixed acid to heat so as to obtain pure and colourless nitric acid, as it may be used for almost all the purposes to which the latter is applied. It is even preferred in many processes from its great strength, being always stronger and more active than the pale acid. Dr. Hope has prepared it with so high a specific gravity as 1·54, though the specific gravity of the colourless acid does not exceed 1·500." Others have obtained the red acid of a still higher specific gravity than Dr. Hope; but whatever may be its density, we deny that it is on that account stronger than pale acid, nor are we aware that it is more active; and if it were so, the proof would be difficult, for it is never used as a solvent without previous dilution.

When nitric oxide gas is passed into colourless nitric acid it becomes red, and its density is considerably increased: when, however, water is added, which must be done before use, the nitric oxide is expelled, leaving the acid without either increase or diminution of solvent power: if then we use equal weights of concentrated pale and red acid, the former will be the stronger. We think we can also defy Mr. Reid to point out a single process, in which the coloured acid is to be preferred to the pale: we know that

that Dr. Hope has already made a similar assertion, but it was unaccompanied by proof, and the Edinburgh Pharmacopœia certainly does not contain any.

At page 60 our author states that "nitric acid (by which the common liquid nitric acid is always understood, composed of one equivalent of real acid and two of water,") possesses certain properties, all of which it is not requisite to quote; but he tells us that "it is distinguished from all other acids by the facility with which it affords oxygen to metals and combustible bodies, most of which decompose it with great rapidity." This the reader will remember is stated of acid of sp. gr. 1·5: and yet in direct contradiction to this, and only six lines further on in the same page, we are informed that nitric acid of sp. gr. 1·48 "is scarcely affected by the metals at ordinary temperatures," requiring the addition of a "small quantity of water" to cause its decomposition. Here then occurs another instance of that want of care of which we have given so many;—nitric acid of sp. gr. 1·5, containing of course less water than that of 1·48, is decomposed by metals "with great rapidity;" while that of 1·48, which contains more water than that of 1·50, "is scarcely affected by the metals at ordinary temperatures, without adding a small quantity of water."

Let us then suppose that the acid is to be diluted; yet when it is decomposed by metals "all the oxygen, however, is not withdrawn from the nitrogen, nitric oxide, and nitrous acid, being generally disengaged during the action that takes place." These statements are, we apprehend, not quite correct: we admit that nitric oxide is usually evolved, and sometimes nitrous oxide is mixed with it; but the nature of the gases depends upon the strength of the acid and the nature of the metal; for zinc and very dilute nitric acid, give nitrous oxide nearly pure and unmixed with nitric oxide; and we much doubt whether nitrous acid is in any case whatever evolved. Has not Mr. Reid mistaken the production of nitrous acid, by the action of nitric oxide upon the oxygen of the atmosphere, for its direct evolution by the action of the metal?

On the subject of the action of dilute nitric acid upon the metals, Mr. Reid has, if we mistake not, committed another error. After stating that the nitric acid and water are both decomposed, he says: "When this takes place, the hydrogen of the water unites with the nitrogen of the acid forming ammonia, which combines with part of the nitric acid that is not decomposed, forming nitrate of ammonia: this explains," continues Mr. Reid, "the appearance of the white fumes which are often seen intimately blended with the nitrous acid vapours that are formed when this acid is decomposed by a metal having a great affinity for oxygen."

For our part, we must confess that we had never observed this blending of white and red vapours during the solution of a metal; but *fiat experimentum* is our motto, and we accordingly mixed some diluted nitric acid and tin; and we do admit that for a moment, and by the action of the heat generated before any considerable evolution of nitric oxide occurred, that some white vapours were

were visible; they were, however, instantaneously succeeded by red fumes, in which it required powers of vision superior to those of second sight to detect any blending of white vapour.

We suppose that Mr. Reid takes the white vapour for volatilized nitrate of ammonia: but he must surely be aware that this salt is decomposed at a low temperature and not vaporized; and in proof that it is neither decomposed nor volatilized in the present instance, it is well known that when lime is added to the oxide of tin, the nitrate of ammonia diffused through it is decomposed and the alkali evolved.

Mr. Reid next notices the action of nitric oxide upon nitric acid, and the changes of colour which result from it. "If a current of nitric oxide gas be transmitted through colourless nitric acid, a large quantity of this gas is absorbed, and the acid speedily acquires a light-straw colour, which deepens to a reddish brown, and passes through various shades of olive and green, till it at last becomes almost blue." From this statement, it would appear that the only circumstance which determines the colour of the acid, is the quantity of nitric oxide which it absorbs. We have been already instructed to consider, that by nitric acid we are to understand that which is concentrated, and such we presume is that to be employed in this experiment; and if this be the case, the effects produced are described with extreme and most culpable inaccuracy. In the first place, strong nitric acid never becomes at all either olive, green, or blue by absorbing nitric oxide: and what proves that Mr. Reid never made the experiment is, that when the colours are produced they do not occur in the order stated by him; nor is acid of any one degree of strength capable of exhibiting them all, whatever may be the quantity of nitric oxide passed into it. If, for example, pale acid of sp. gr. 1·46 be used, the nitric oxide renders it for a moment yellowish, and then it becomes red, but never olive, green, nor blue at all: dilute a portion of this acid with an equal bulk of water, the nitric oxide then renders it green, but it assumes no other colour; mix three measures of the acid with four of water, and the mixture may be rendered blue, but neither yellow, olive, red, nor green. The mistakes committed by Mr. Reid in this part of the subject are the more remarkable, because in the very next page to that in which they occur, he shows that the production of the different colours does not depend upon the quantity of nitric oxide absorbed by strong acid, but upon that retained after various degrees of dilution; for he says, if small quantities of water be added to the strong fuming acid, "it gradually loses its deep orange red colour and passes through various shades of olive, green and blue; and if a sufficient quantity of water be added, it becomes quite colourless."

"Pure nitric acid," according to our author, "is easily recognised by the facility with which it is decomposed by inflammable substances and the metals, especially zinc, tin, mercury and copper, and the large quantity of deep ruddy fumes which are disengaged." Now this is a method of ascertaining the purity of nitric acid

acid which, we believe, never occurred to any chemist before. Mr. Reid surely does not mean to state that an admixture of sulphuric or muriatic acid would prevent the action of the nitric acid so contaminated; on the contrary, he must know that there is a "lovely experiment;"—we mean the accension of oil of turpentine by nitric acid, in which it is common to add sulphuric acid to it, to accelerate the combustion. We really suspect that the word *pure* has crept in here by mistake; and yet something is wanting to account for the introduction of the remainder of the paragraph, for we have been before told of the mutual action of nitric acid, metals, and combustibles.

Continuing the quotation, we learn that pure nitric acid, "when present in small quantity, however, is not so easily recognised, a there are no substances with which it forms characteristic and insoluble precipitates. The only test indeed which can be relied on, is that proposed by Dr. Liebig (*Ann. de Chimie*, xxxv. p. 80.): he recommends the liquid under examination to be mixed with a solution of indigo in sulphuric acid till it requires [acquires] a perceptible blue colour: a few drops of sulphuric acid are then added to the solution, and the whole is boiled for a short time. If the liquid contain any nitric acid it will be completely deprived of colour, or rendered yellow if the proportion of acid is extremely small."

We presume that Mr. Reid will not insist that the acid must be pure in order that its presence may be detected, though such is the legitimate inference deducible from his statement. We may also observe that Liebig's process is not intended for the detection of uncombined nitric acid, as Mr. Reid supposes, but of a nitrate; and the use of the sulphuric acid is to separate the nitric acid from its base, that it may act upon the indigo. Our author then informs us that another very delicate test consists in subjecting a solution, suspected to contain a nitrate, to the action of sulphuric and muriatic acid and gold-leaf; adding, "this test, however, cannot be relied on in all cases, as chloric acid produces the same effect when treated in this manner." Now we would simply inquire whether the acid of a chlorate would not by suffering decomposition also discharge the colour of indigo? Mr. Reid must admit this, for chlorine is notoriously employed for this purpose; there can therefore be no difference in the accuracy of the two modes, as far as the action of chloric acid is likely to interfere.

We have now arrived at the concluding paragraph of the chapter on Nitric Acid; it is as follows: "Oxygenated nitric acid may be prepared much in the same manner as oxygenated water, the deutoxide of barium being dissolved in diluted nitric acid, the barytes removed by sulphuric acid, and the liquid that remains strengthened by evaporation in the exhausted receiver of the air-pump. It presents the same general phænomena with many of the metals and metallic oxides, &c. as oxygenated water, but has not hitherto been applied to any use." In the year 1818 (Annals of Philosophy, vol. xv. p. 378), M. Thenard announced to the Academy of

of Sciences, that "by carefully dissolving superoxidized barytes in nitric acid, and precipitating the barytes from it by sulphuric acid, the excess of oxygen remains united with the former acid, which by this means becomes oxygenized nitric acid." How soon M. Thenard discovered that he had mistaken the nature of the compound formed, we do not know; but in the fourth edition of his *Traité de Chimie*, published in 1824, he says (vol. ii. p. 83), "Puisque les acides donnent plus de stabilité à l'eau oxigénée, c'est sans doute en se combinant avec le peroxide d'hydrogène: du moins, dans l'état actuel de la chimie, la composition de ce peroxide rend toute autre hypothèse invraisemblable. A' la vérité, cette opinion n'est pas celle que j'avais adoptée d'abord: j'avais pensé que l'oxigène se combinait avec les acides, et qu'il en résultait un grand nombre de nouveaux acides oxigénées." As the result of further experiments he concludes: "je compris et je reconnus bientôt que ce qui m'avait paru être des acides oxigénées n'etait que l'eau oxigénée et acidifiée." It is clear, therefore, even in the opinion of Thenard himself, that no such compound as oxygenated nitric acid exists: and it is singular that Mr. Reid should have stated that as a fact, which had been six years at least exploded by its promulgator; more especially as there is not, that we know of, one work on the subject of chemistry written during that period, which contains any notice of the existence of such an acid.

The circumstance which we have just pointed out, as well as the numerous mistakes that have been previously detected, prove that Mr. Reid is not sufficiently acquainted with the science which he has undertaken to teach: and even when he appears to possess some knowledge of facts, they are usually very loosely, imperfectly and incorrectly stated. We might adduce numerous instances to those already given in support of these charges, but we shall now conclude, with adding, that the work is very inaccurately printed; for in the chapter which we have examined, there occur four typographical errors, only one of which has been corrected; and we have detected several in other parts of the work, that are not included in the Errata.

LXX. *Proceedings of Learned Societies.*

ASTRONOMICAL SOCIETY.

May 14.—THE following communications were read:—

1. On an appearance of divisions in the exterior ring of Saturn. By Captain Henry Kater, Vice-President and Treasurer of the Royal Society.

The observations which form the subject of this paper were made in the years 1825 and 1826, and remained unpublished from a wish on the part of the observer to witness the appearances again,—but bad health and the uncertain state of the atmosphere in this country have hitherto prevented him.

The planet Saturn has been much observed by Captain Kater, for

for the purpose of trying the light, &c. of his telescopes, for which the ring and satellites are good tests. The instruments which were employed in the present investigations were two Newtonian reflectors,—one, supposed by Watson, of 40 inches focus, and 6¼ aperture; and another, by Dollond, of 68 inches focus, and 6¼ aperture. The first, under favourable circumstances, gives a most excellent image; the latter is a very good instrument. The following are extracts from the author's journal.

"Nov. 25, 1825.—The double ring beautifully defined, perfectly distinct all round, and the principal belts well seen. I tried many concave lenses, and found that the image was much sharper than with convex eye-glasses, and the light apparently much greater. Dollond, 259 the best power; 480, a single lens, very distinct.

"Nov. 30.—The night very favourable, but not equal to the 25th. The exterior ring of Saturn is not so bright as the interior, and the interior is less bright close to the edge next the planet. The inner edge appears more yellow than the rest of the ring, and nearer in colour to the body of the planet.

"Dec. 17.—The evening extremely fine. With Dollond I perceived the outer ring of Saturn to be darker than the inner, and the division of the ring all round, with perfect distinctness; but with Watson I fancied that I saw *the outer ring separated by numerous dark divisions extremely close, one stronger than the rest dividing the ring about equally*. This was seen with my most perfect single eye-glass power. A careful examination of some hours confirmed this opinion.

"Jan. 16 and 17, 1826.—Captain Kater believed that he saw the divisions with the Dollond, but was not positive. Concave eye-glasses found to be superior to convex.

"Feb. 26, 1826.—The division of the outer ring not seen with Dollond.

"Jan. 22, 1828.—The evening remarkably fine, and the same appearances shown as on the 30th November, 1825, but no divisions seen in the outer ring. I am, therefore, the more persuaded that they are not permanent." With the Dollond.

On the 17th December, when the divisions were most distinctly seen, Captain Kater made a drawing of the appearance of Saturn and his rings. The phænomena were witnessed by two other persons on the same evening, one of whom saw several divisions in the outer ring, while the other saw one middle division only; but the latter person was short-sighted, and unaccustomed to telescopic observations.

It will be remarked, that these divisions were not seen on other evenings, which yet were considered very favourable for distinct vision.

It seems that the same appearances were seen by Mr. Short, but the original record of his observations cannot be found.

In Lalande's Astronomy (third edition), article 3351, it is said, "Cassini remarked that the breadth of the ring was divided into two equal parts by a dark line having the same curvature as the ring,

ring, and that the *exterior* portion was the less bright." (*Mém.* 1715, p. 13.) Short *told* me that he observed still more singular phænomena with his large telescope of 12 feet.

The breadth of the ansæ, or extremities of the ring, was, according to him, divided into two parts,—an inner portion without any break in the illumination, and an outer, divided by several lines concentric with the circumference; which would lead to a belief *that there are several rings in the same plane.*

Delambre and Biot severally state, that Short saw the outer ring divided, probably on the authority of Lalande cited above. There is a note to the same purpose in Brewster's edition of Ferguson's Astronomy, but without reference to the source from which it is taken.

In December 1813, at Paris, Professor Quetelet saw the outer ring divided, with the achromatic telescope of 10 inches aperture, which was exhibited at the exposition. He mentioned this the following day to M. de la Place, who observed, that " those, or even more divisions, were conformable to the system of the world."

But, on the other hand, the division of the outer ring was not seen by Sir William Herschel (Phil. Trans. 1792), nor by Mr. Herschel in 1826, nor by M. Struve in the same year; and on several occasions when the atmospheric conditions were most favourable, it has not been seen by Captain Kater.

" It has been remarked by Sir William Herschel, by M. Struve, and by most persons who have observed Saturn, that the exterior ring is much less brilliant than the interior. May not this want of light in the outer ring arise from its having a very dense atmosphere? and may not this atmosphere in certain states admit of the divisions of the exterior ring being seen, though, under other circumstances, they remain invisible?

" With respect to the form of the edge of the inner ring of Saturn next to the planet, the appearance under favourable circumstances is such as to leave no doubt on my mind of its being rounded."

2. Occultations observed at Biggleswade with the Society's Wollaston telescope. By T. Maclear, Esq., member of the Society.

3. On the Comet discovered at Marseilles in the constellation Equuleus. By James South, Esq. President.

4. Notice of the performance of the 20-feet achromatic, May 14, 1830.

" *Georgium Sidus.*—At half-past two, placed the 20-feet achromatic on the Georgium Sidus; saw it with 346, a beautiful planetary disk; not the slightest suspicion of any ring, either perpendicular or horizontal; but the planet three hours east of the meridian, and very low. Morning beautiful, but moonlight very strong, and the moon within three degrees of the planet.

" *Jupiter* (at a quarter before three) with 252 and 346, literally covered with belts, and the diameters of his satellites might have been as easily measured as himself. One came from behind the body, and the contrast of colour with that of the planet's limb was striking.

" *Mars*

"*Mars* (near three o'clock).—The contrast of light in the vicinity of the poles very decided; on the 3rd, several spots on his body, well and strongly marked. This morning, that about the south pole seems to overtake the body of the planet, and gives an appearance not unlike that afforded by the new moon, familiarly known as 'the old moon in the new moon's arms.'

"*Saturn* has been seen repeatedly with powers 130 to 928, under circumstances the most favourable; but not anything anomalous about the planet or its ring could even be suspected."

June 11.—The following communications were read:

I. Observations and remarks made on a passage from New South Wales to England, by Charles Rumker, Esq., Member of the Society.

1st. A short table, with directions for ascertaining the time approximately by the position of the constellation of the Cross. For a rough estimation, it may be remembered that the Cross is vertical above the pole at midnight 21st March, and vertical below the pole at midnight Sept. 23rd.

2nd. Observations of the variation and dip of the needle, made on the passage home in 1829, round Cape Horn: and,

3rd. Currents.

"The waters of the Pacific being supposed higher than those of the Atlantic, I expected an easterly current on approaching Cape Horn, but I could discover none. Near the northern coast of the Brazils and Guiana we experienced strong currents to the west, in conformity with Humboldt's theory of an indraft into the Caribbean Sea, occasioned by an equatorial current." Mr. R. gives a table of comparison between the ship's place by observation and that obtained by the log, with the drift and force of the current for twenty-four hours.

4th. "*Sargasso Weeds.*—In the North Atlantic Ocean, coming from the south, you fall in about the tropic with the Sargasso weeds, collected in narrow lines extending in the direction in which the trade wind blows, that is, E.N.E. and W.S.W., and the eye cannot see the end of them on either side of the vessel. These lines run constantly parallel to each other, and the nearer you come to the middle of the Sargasso Sea the thicker it is strewed with weeds, and the closer the lines approach to one another, being in some places but fifteen feet asunder. Home-bound ships have a better opportunity of observing these lines, as they cross them nearly at right angles, and can trace their continuation more conveniently on both sides, observing one line after another in rapid succession.

"These weeds occupy the zone from about 20° to 35° north latitude, which may, however, differ according to the longitude in which you cross it. Towards the zone's northern extreme the weeds are less regularly formed in lines, which may arise from their being less methodically acted upon by the trade-winds that seem to occasion their order. They have been termed gulf-weeds by sailors, who believed them to be driven out of the gulf by the Florida stream; nor is this opinion entirely refuted by the experience that they are

rarely met with in the gulf. For the weed swimming on the surface of the Atlantic is withered, decayed, and incrusted with salt, which proves the time it has been exposed to the sun, and is of a brownish-yellow colour, whilst you rarely meet with a green bunch: that, being heavier on account of its higher state of vegetation, swims several feet below the surface. It is true that not with certainty can any roots, thicker branches, or stems, be perceived, wherewith they might have adhered to rocks or the ground: nevertheless, as these weeds abound with animals that do not live upon the surface, but inhabit the bottom of the sea, such as crabs, shrimps, barnacles, conchilias of all descriptions, and serpents, I have no doubt that they originated in a shallow basin of water, out of which they were swept by the force of a current along the bottom, until the heavier vegetable fluid being exhausted, they rose to the surface. Moreover, they are never seen near the European or African coast, but most plentifully found about the entrance of the gulf."

II. A letter from F. Hartmann, lieutenant of engineers, to J. F. W. Herschel, Esq., describing an instrument constructed for him, according to his own directions, by M. Hohnbaum, optician, of Hanover.

III. On the rectification and use of the equatorial. By M. Kreil.

This paper is intended to serve as a continuation and practical application of one by Professor Littrow on the same subject, published in the Ast. Soc. Memoirs, vol. ii. p. 45.

IV. On Barlow's new telescopes. By Professor Littrow.

V. A letter by Mr. Hubert to the President, suggesting the possibility that the discrepancies observed in pendulum experiments may arise from the effect of magnetic attraction. He recommends the pendulum to be swung, at the same place, in the magnetic meridian, and at right angles to it, in order to ascertain the fact; and that observers should be carefui to swing the pendulum in the magnetic meridian for comparative observations.

VI. On Mayer's celebrated Catalogue of Zodiacal Stars, by Francis Baily, Esq.

Since the publication of the Astronomical Observations of Mayer, by the late Board of Longitude, Mr. Baily has examined the position of every star, by comparing each of them not only with the observations, but also with the catalogues of Bradley and Piazzi, by which means he has been enabled to detect many inaccuracies in Mayer's catalogue, which have hitherto puzzled the practical astronomer; and he has now given us a corrected catalogue, together with references to every observation of every star, in a collateral column, as well as the difference between the results of Mayer's observations and those of Bradley, his contemporary, in another collateral column. The corrections which Mr. Baily has actually introduced are those only which are sanctioned by the observations themselves: at the same time, he has pointed out other corrections, which, although only conjectural, are highly probable; but these are kept distinct from the former. The author has subjoined a list of some other stars observed by Mayer, but not contained in his catalogue, as well as a list of several occultations. From the result of Mr. Baily's investigations,

tions, it appears that there is a very near accordance between Bradley and Mayer in the declination of the stars; and it is a remarkable fact, that the differences are almost always on the same side: the mean of all the comparison is 4″,27, which is the quantity by which Bradley's north polar distances exceed those of Mayer. There is a greater discordancy, as might be expected, in the right ascensions.

VII. A paper from Dr. Robinson, of Armagh, containing the results of his observations of Venus, for the purpose of determining thereby the mass of the Moon, in the way proposed by Professor Airy.

VIII. Two Memoirs by Don José Joachim de Ferrer, containing Observations made at the Havannah, from 1808 to 1812, with the resulting longitude and latitude of that city.

The latitude of the observatory of the Havannah is 23° 8′ 17″,5 N., and that of the great tower of the Moro 23° 9′ 26″,2.

The mean of 20 results gives the longitude of the Havannah west of Paris $5^h\ 38^m\ 47^s,7$, no observation differing 10^s from the mean. The great tower of the Moro is $1^s,6$ west of the observatory.

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

Seventeenth Annual Report of the Council.

[The Council, after noticing the demise of His late Majesty, who originally took the Society under his protection, as Patron, during the period of his Regency, and also the accession of his Royal Brother to the Throne, and suggesting an address of condolence and congratulation, proceed as follows:]

The communications which have been made to the Society since the publication of its third volume of Transactions being quite sufficient to fill another volume, the Council suggest that an immediate arrangement be made for the printing and publication of a fourth volume: and as the Society will probably take one hundred copies for its members, those who may wish to be furnished with copies will intimate their wish to the Secretary.*

The Council cannot refrain from expressing the pleasure which they feel in communicating to the Society a proposal from Dr. Paris, its Founder, made to them in person on this day, viz. To deposit with the President the sum of Ten Guineas, to be laid out on a Medal, to be presented to the writer of the best practical communication on Mining, to be made to the Society at their next annual meeting.

The donations of metallic and earthy minerals have not been great during the last year; but for those which have been received, the Society are indebted to the kindness of Lieutenant General Tench, Mr. Alfred Fox, Mr. Joseph Carne, &c. In the department of Geology, however, the Council have to congratulate the Meeting upon a large and interesting series: their late highly respected Secretary Dr. Boase has during the past summer investigated the geological structure of the north, and a considerable part of the centre of the county; he has traced the rock formations of Pydar,

* Agreeably to this suggestion, it was resolved by the Society, "That a fourth volume of Transactions be immediately published."

Trigg,

Trigg, Lesnewth, Stratton, and Powder, and penetrated some distance into the hundreds of east and west. He has presented the Society with a memoir, containing the results of these researches, accompanied by a beautiful section of the different districts, and a series of illustrative specimens. This valuable contribution will not only form a very prominent feature in the Geology of Cornwall, but if followed up, would soon accomplish the map, which has been so long in contemplation.

The Council also beg to refer the Meeting to a series of specimens from the Garth Mine, in illustration of Mr. Carne's paper upon Diluvial Tin, and presented by him to the Society. (By Order)

October 6th, 1830. E. C. Giddy, Secretary.

The following is a list of the papers which have been read since the last meeting:—

On the very singular deposit of Alluvial Matter on St. Agnes Beacon, and the Granitic Rock which occurs in the same situation. By John Hawkins, Esq. F.R.S. &c. Member of the Society.—A Description of the Stream Work at Drift Moor, near Penzance. By Joseph Carne, Esq. F.R.S. F.G.S. M.R.I.A. &c. Treasurer of the Society.—Some Account of the Soft Growan at Beam Mine, in the parish of Roche; and at Carclaze Mine, in the parish of St. Austle. By John Hawkins, Esq.—Some Observations on Metalliferous Veins, and their Electro-magnetic Properties. By Robert Were Fox, Esq. Member of the Society.—Remarks on Alpine Phænomena. By the Rev. S. J. Trist.—On Diluvial Tin, with a notice of the discovery of some varieties of the Oxide of Tin in a vein, which have been generally supposed to be peculiar to Stream Works. By Joseph Carne, Esq. —Contributions towards a Knowledge of the Geology of Cornwall. By Henry S. Boase, M.D. Member of the Society.—Observations on some of the Mineral Veins of Cornwall; and on some of the Metalliferous Deposits of Devon. By Mr. W. J. Henwood, F.G.S. Member of the Society.—On the State of our Tin Mines, at different periods, until the commencement of the eighteenth century. By John Hawkins, Esq.—An Account of the Quantity of Tin produced in Cornwall and Devon, in the year ending with Midsummer Quarter, 1830. By Joseph Carne, Esq.—An Account of the Quantity of Copper produced in Cornwall, and in Great Britain and Ireland, in the year ending the 30th June, 1830. By Mr. Alfred Jenkin.

DONATIONS TO THE CABINET AND LIBRARY.

Specimens from Brazil, consisting of Topaz, Amethyst, Semi-Opal, Rock Crystal, Gold imbedded in micaceous Iron, &c. By Lieutenant General Tench.—Native and grey Copper, Nodules containing Quartz Crystals, &c. By Joseph Carne, Esq.—Carbonate of Manganese, and Varvicite, a New Oxide of Manganese*. By Alfred Fox, Esq. —Specimens of Silver Glance from Mexico. By Mr. John Adams. —Specimen of Slate traversed by a vein of Yellow Sulphuret of Copper. By Mr. James Mitchell.—Additional Specimens of Organic remains from the neighbourhood of Torquay. By the Rev. J. M'Enery.

* See Phil. Mag. and Annals, N. S. vol. v. p. 209, 254; vol. vi. p. 281.

—A

—A series, consisting of more than 300 Rock specimens, illustrative of the Geology of the Northern and Central part of Cornwall. By Henry S. Boase, M.D.—Three additional specimens from the neighbourhood of Liskeard. By the Rev. Canon Rogers.—A series of specimens in illustration of the Geology of Dartmoor, with a memoir. By John Prideaux, Esq. of Plymouth.—A series of Rocks and Organic Remains from Yorkshire. By the Yorkshire Philosophical Society.—Specimens from the Manganese Beds near Tavistock, and some specimens of Greenstone Slate; also, specimens of Schorl Rock from Park Wood. By Mr. W. J. Henwood.—A series of specimens from the Garth Mine. By Joseph Carne, Esq.—Quarterly Mining Review, Nos. 1, 2, 3. By Henry English, Esq. the editor.—Geological Notes. By the author, Henry Thomas De la Beche, Esq. F.R.S. &c.—Asiatic Researches. By the Asiatic Society.

Officers and Council for the present year:—*President:* Davies Gilbert, Esq. M.P. P.R.S., &c. &c.—*Vice-Presidents*. The Earl of Falmouth; Sir Rose Price, Bart.; John Hawkins, Esq.; Francis Hearle Rodd, Esq.—*Secretary:* E. C. Giddy, Esq.—*Treasurer:* Joseph Carne, Esq.—*Librarian:* Thomas Hingston, M.D.—*Assistant Secretary:* R. Moyle, Esq.—*Council:* Doctor Boase; Wm. Cornish, Esq.; Richard Davey, Esq.; Day P. Le Grice, Esq.; H. M. Grylls, Esq.; Alfred Fox, Esq.; James Plomer, Esq.; Wm. Reynolds, Esq.; Wm. M. Tweedy, Esq.; Wm. Williams, Esq.

New Members: Edward Collins, Esq. Truthan; the Rev. Michael Noel Peters, Penzance; and Charles Fox, Esq. Perran Wharf.

LXXI. *Intelligence and Miscellaneous Articles.*

ON THE SESQUIPERSULPHATE OF MERCURY. BY LIEUT. W. T. HOPKINS, UNITED STATES.

"Having had occasion, lately, to pour strong nitric acid on the yellow neutral persulphate of mercury, known as the 'turpeth mineral,' I observed that a portion of the salt disappeared, while the remainder was converted into a white powder. This substance was readily reconverted by water into the yellow salt. I succeeded however in edulcorating a portion of it without change; and upon boiling thirty grains of it with nitrate of baryta, digesting afterward with nitric acid, (to remove a portion of peroxide of mercury which was deposited,) washing, drying, and weighing, I obtained 19 grains of sulphate of baryta. Now 19 grains of sulphate of baryta contain 6·44 grs. of sulphuric acid, and the white powder submitted to experiment consisted of sulphuric acid, 6·44 grs. + peroxide of mercury, 23·56 grs. = 30. We have therefore the proportion

$$23{\cdot}56 : 6{\cdot}44 :: 216 : 59{\cdot}04$$

where 216 represents the atom of peroxide of mercury, and 59·04 the quantity of sulphuric acid that would combine with it to produce the compound under examination. If the numbers I had obtained had been 6·48 and 23·52, the proportion would have been

$$23{\cdot}52 : 6{\cdot}48 :: 216 : 60$$

and

and this last term would precisely represent $1\frac{1}{2}$ atom of sulphuric acid. The hastiness of my experiment could account for a much greater discrepancy.

This sesquisulphate has no very interesting properties. It appears to be insoluble, but is easily decomposed by cold water. It is usually said that the action of water upon the white bipersulphate of mercury consists in the resolution of it into the yellow, insoluble, neutral sulphate, and a soluble supersulphate. Yet upon evaporating the liquor thus obtained, I observed the deposition of a white substance resembling in its external characters the bipersulphate; while the supernatant liquid had those of free sulphuric acid."—*Silliman's Journal,* vol. xviii. p. 364.

ON CHLORIDE OF SILVER. BY M. CAVALIER.

The colour produced in chloride of silver by the action of light has long been known, and a similar change is apparently produced by some chemical reagents, but whether the alterations are identical is a question which M. Cavalier says he does not pretend to decide; he then states a method by which the violet chloride of silver may be procured without the agency of light: Dissolve some recently prepared and perfectly white chloride of silver in ammonia, and pass a current of chlorine gas through it, and the same phænomena as occur when the gas is passed through mere solution of ammonia will be presented; such as slight detonation on the arrival of each bubble, abundant white vapours, increase of temperature, and the disengagement of azotic gas, &c. Afterwards the solution becomes turbid, and soon a grayish precipitate is observed, and at length it assumes a well marked violet colour; this colour occurs when the ammonia is completely decomposed by the chlorine.

What is the nature of this new substance? Is it a smaller or greater quantity of chlorine which has modified the properties of the chloride, or is it identical with the white chloride; and is the colour acquired merely by a different molecular arrangement?

The following experiments are in favour of the latter opinion.

If the violet chloride be dissolved in ammonia, nitric acid precipitates it white. Take 20 grains of violet and 20 grains of white chloride, put each into a glass and with them diluted sulphuric acid and a piece of zinc, stirring the chloride with the latter so as to keep it suspended; the chlorides are both decomposed by the hydrogen evolved and metallic silver is obtained, and from each chloride the same quantity, viz. 15 grains.

According to these experiments, the new substance cannot be regarded either as a subchloride or a deutochloride; every circumstance seems to prove that the colour is produced merely by a different molecular arrangement. In this case it remains to be explained what is the body which forces the chloride to acquire a different physical property. The heat produced during the operation has certainly nothing to do with it, for the experiment succeeds equally when the vessel is placed in a freezing mixture.—*Journal de Pharmacie,* xvi. 552.

AURORA

AURORA BOREALIS.

At 20 minutes past 7 o'clock in the evening of the 5th of October, an aurora borealis suddenly rose between the N. by E. and N.W., a little above the edge of a dark *cirrostratus* cloud, which was seven or eight degrees above the horizon: after remaining a steady arc about ten minutes, several thin columns of light emanated from it, and two meteors passed under Ursa Major. A few minutes before eight, two or three more coruscations rose from the aurora, which was perceptible till after the moon rose.

At ten o'clock in the evening of the 16th an aurora borealis again appeared, from which several wide red columns emanated between the true and magnetic north, and rose to the same altitude as the star β in Ursa Major. The aurora disappeared in half an hour, and one meteor fell near the star Benetnasch. It also appeared the following evening, but without any coruscations; and two meteors appeared over it.

Gosport.

OCCULTATION OF ALDEBARAN BY THE MOON.

In the morning of the 6th of October, at $6^h\ 46^m\ 21^s$ apparent time, an immersion of Aldebaran behind the moon's enlightened limb, about 22° on the right of her vertex, was observed at Gosport. Aldebaran appeared on the moon's limb nine or ten seconds before it finally disappeared, but this unusual time was owing in a great measure to the oblique immersion of the star, and the consequent small chord which it had apparently to describe.

The exact time of the emersion was not observed, it having taken place several minutes sooner than the computed time; but it could not have exceeded a few seconds before or after $7^h\ 2^m$ apparent time here. Missing the time of the emersion was regretted, as it might have been of some service to those who had taken the trouble to compute the occultation.

METEOROLOGICAL OBSERVATIONS FOR OCTOBER 1830.

Gosport:—Numerical Results for the Month.

Barom. Max. 30·51. Oct. 9. Wind N.E.—Min. 29·76. Oct. 29. Wind W.
Range of the mercury 0·75.
Mean barometrical pressure for the month .... 30·235
Spaces described by the rising and falling of the mercury........... 4·260
Greatest variation in 24 hours 0·430.—Number of changes 18.
Therm. Max. 65°. Oct. 22. Wind S.W.—Min. 36°. Oct. 26. Wind N.
Range 29°.—Mean temp. of exter. air 53°·24. For 31 days with ⊙ in ♎ 54·21
Max. var. in 24 hours 21°·00.—Mean temp. of spring-water at 8 A.M. 53·32

De Luc's *Whalebone Hygrometer.*

	°
Greatest humidity of the atmosphere, in the morning of the 25th...	95·0
Greatest dryness of the atmosphere, in the afternoon of the 13th...	57·0
Range of the index	38·0

Mean

Mean at 2 P.M. 65°·8.—Mean at 8 A.M. 75°·3.—Mean at 8 P.M. 77·0
—— of three observations each day at 8, 2, and 8 o'clock .... 72·7
Evaporation for the month 1·75 inches.
Rain in the pluviameter near the ground 0·595 inches.
Prevailing winds, N.E. and N.W.

Summary of the Weather.

A clear sky, 8; fine, with various modifications of clouds, 12; an overcast sky without rain, 9; rain, 2.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
22	8	27	3	15	13	8

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
3	5½	4	3½	1	4	4½	5½	31

General Observations.—This has been the finest and driest October that we have had for many years past, but not the warmest. From the 3rd to the 22nd no rain fell here; the atmosphere was often transparent and cloudless, and its elasticity very seldom broken by the wind; therefore the mercury in the barometer maintained a high elevation till the 28th. Both the *maximum* and *minimum* temperatures have been comparatively high and low for several days during the month, and were thus influenced chiefly by the position of the prevailing winds. The 19th, 20th, 21st, and 22nd were warm days and nights, but were preceded and followed by a cold air.

The mean temperature of the external air this month is one-seventh of a degree higher than the mean of October for many years.

The atmospheric and meteoric phænomena that have come within our observations, are four parhelia, two solar and two lunar halos, fourteen meteors, three auroræ boreales, and three gales of wind, namely, two from the West, and one from the North-west.

REMARKS.

London.—October 1. Fine. 2. Fine: slight rain at night. 3. Slight rain in the morning: dull. 4, 5. Fine. 6. Cloudy. 7. Fine. 8. Foggy in the morning: fine. 9—11. Fine. 12. Cloudy: slight rain at night. 13—18. Foggy in the mornings: clear and fine through the day. 19. Slight rain: fine. 20, 21. Very fine. 22. Fine: rain in the evening. 23. Cloudy. 24. Cloudy: rain at night. 25. Rain, with brisk wind. 26, 27. Cloudy: rain at nights. 28. Stormy and wet. 29, 30. Fine. 31. Rain in the morning: fine.

Penzance.—October 1. Misty: fair. 2. Rain. 3. Fair: showers. 4—10. Fair. 11, 12. Clear. 13. Fair. 14—16. Clear. 17, 18. Fair. 19. Rain: fair. 20, 21. Fair. 22. Rain: fair. 23. Fair. 24. Fair: rain. 25. Rain. 26. Fair. 27. Clear: fair. 28. Rain. 29. Showers. 30, 31. Rain: fair.

Boston.—October 1. Cloudy. 2. Fine. 3. Cloudy: rain early A.M. 4. Cloudy. 5. Fine. 6. Cloudy. 7. Fine. 8, 9, Cloudy. 10. Fine. 11. Cloudy. 12, 13. Fine. 14. Cloudy. 15—18. Fine. 19. Fine: rain at night. 20, 21. Fine. 22. Cloudy: rain P.M. 23. Fine. 24. Cloudy: rain at night. 25. Cloudy: rain A.M. and P.M. 26. Fine. 27. Fine: rain P.M. 28. Cloudy: rain P.M. 29, 30. Fine. 31. Rain.

Meteoro-

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY at Penzance, Dr. BURNEY at Gosport, and Mr. VEALL at Boston.

Days of Month, 1830.	Barometer.							Thermometer.							Wind.				Evap.	Rain.			
	London.		Penzance.		Gosport.		Boston	London.		Penzance.		Gosport.		Bost.									
	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	Max.	Min.	8½ A.M.	Lond.	Penz.	Gosp.	Bost.	Gosp.	Lond.	Penz.	Gosp.	Bost.
Oct. 1	30·100	30·070	29·95	29·90	30·09	30·09	29·51	62	52	60	48	59	57	50·5	S.	SW.	SW.	W.	...	...	...	0·080	...
○ 2	30·110	30·000	29·90	29·85	30·09	30·01	29·50	65	55	61	49	62	57	56	S.	S.	S.	W.	...	0·04	0·660	·100	...
3	30·113	30·000	30·00	30·00	30·09	29·97	29·35	63	50	60	54	62	53	58	S.	NW.	SW.	W.	0·20	...	·030	·010	0·02
4	30·368	30·158	30·20	30·15	30·37	30·16	29·37	65	38	58	50	63	42	57	NW.	N.	W.	W.	...	...	...	...	...
5	30·440	30·416	30·40	30·40	30·45	30·43	29·83	62	41	57	48	59	42	51	N.	NE.	N.	NW.	...	...	...	...	...
6	30·418	30·364	30·39	30·38	30·40	30·37	29·77	56	41	57	47	57	50	53	NW.	NE.	NW.	NW.	·20	...	...	...	...
7	30·492	30·392	30·30	30·30	30·42	30·36	29·77	66	45	57	47	61	46	55·5	SW.	NE.	NW.	NW.	...	...	...	...	...
☾ 8	30·502	30·484	30·38	30·35	30·48	30·45	29·85	67	47	60	52	61	46	52·5	W.	NE.	NW.	NW.	...	...	...	...	...
9	30·541	30·529	30·40	30·35	30·51	30·51	29·95	58	45	57	47	58	51	53	NE.	SE.	SE.	calm	·15	...	...	...	...
10	30·550	30·457	30·30	30·25	30·45	30·43	30·00	60	40	58	52	62	47	50	W.	SE.	E.	calm	...	...	...	...	...
11	30·416	30·356	30·20	30·20	30·38	30·33	29·87	58	47	59	53	60	50	51·5	NE.	SE.	NE.	calm	...	...	...	...	...
12	30·368	30·341	30·15	30·15	30·30	30·30	29·84	56	42	59	54	57	48	51·5	NE.	SE.	NE.	calm	·25	·02	...	...	...
13	30·415	30·380	30·17	30·15	30·35	30·34	29·93	62	36	58	51	57	46	50	E.	SE.	NE.	calm	...	...	...	...	...
14	30·352	30·285	30·12	30·10	30·30	30·23	29·80	59	30	56	52	55	43	51	E.	SE.	E.	calm	...	...	...	...	...
15	30·257	30·225	30·03	30·00	30·20	30·18	29·72	58	31	57	47	53	44	44	E.	SE.	E.	calm	·30	...	...	...	...
● 16	30·347	30·289	30·10	30·05	30·30	30·26	29·83	59	32	58	50	55	45	41	E.	SE.	SE.	calm	...	...	...	...	...
17	30·473	30·387	30·20	30·15	30·35	30·32	29·90	55	31	59	50	57	46	47·5	E.	SE.	E.	calm	...	...	...	...	...
18	30·313	30·140	30·10	30·05	30·26	30·13	29·73	62	40	59	49	57	50	52	SE.	SE.	SE.	calm	·20	·02	...	...	...
19	30·034	30·004	29·90	29·88	30·03	30·01	29·46	67	46	60	54	64	54	51·5	S.	SW.	SE.	calm	...	·01	·180	...	...
20	30·122	30·038	29·85	29·82	30·13	30·03	29·36	72	46	62	54	63	55	[illegible]	S.	SW.	SE.	SW.	...	...	...	...	·08
21	30·270	30·150	30·02	30·00	30·26	30·20	29·45	73	44	62	56	64	54	58·5	S.	SW.	SE.	NW.	·15	...	...	...	...
22	30·273	30·264	30·20	30·10	30·33	30·30	29·54	69	52	61	58	65	54	59	S.	SW.	SW.	W.	...	·07	·025	·005	...
23	30·497	30·398	30·35	30·34	30·47	30·40	29·75	61	45	57	48	59	44	52	N.	N.	N.	W.	...	...	...	...	·06
☽ 24	30·489	30·322	30·30	30·20	30·48	30·37	29·83	60	48	56	48	57	51	53	W.	SW.	SW.	calm	·10	·05	·600	·030	...
25	30·090	29·935	30·00	30·00	30·15	30·00	29·50	61	45	58	51	60	44	54·5	SW.	W.	W.	W.	...	·59	·180	·300	·07
26	30·368	30·028	30·32	30·10	30·35	30·10	29·48	54	29	54	48	54	36	44	N.	NE.	N.	NW.	...	·05	...	...	·16
27	30·387	30·182	30·32	30·30	30·37	30·27	29·77	57	41	55	43	53	50	43·5	W.	NW.	W.	NW.	·10	·01	...	·030	...
28	29·964	29·747	30·10	30·00	30·05	29·71	29·30	62	45	56	44	61	47	57	W.	NW.	W.	W.	...	·11	·100	·030	·09
29	29·824	29·685	29·90	29·90	29·77	29·76	29·10	55	36	54	47	53	38	47	W.	NW.	NW.	W.	...	...	·165	...	·13
30	30·085	30·034	30·00	30·00	30·08	30·05	29·54	54	41	55	45	50	45	39	W.	NW.	SW.	W.	...	...	·085	...	...
○ 31	30·081	29·919	30·05	30·00	30·08	30·00	29·40	63	48	57	48	60	50	49	W.	NW.	SW.	SW.	·10	·01	·110	·010	·03
	30·550	29·685	30·40	29·82	30·51	29·76	29·64	73	29	62	43	65	36	51·5					1·75	0·98	2·360	0·595	0·64

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END OF THE EIGHTH VOLUME.

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